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EDITED BY W. S. DALLAS, F.L.S.

Assistant-Secretary of the Geological Society.

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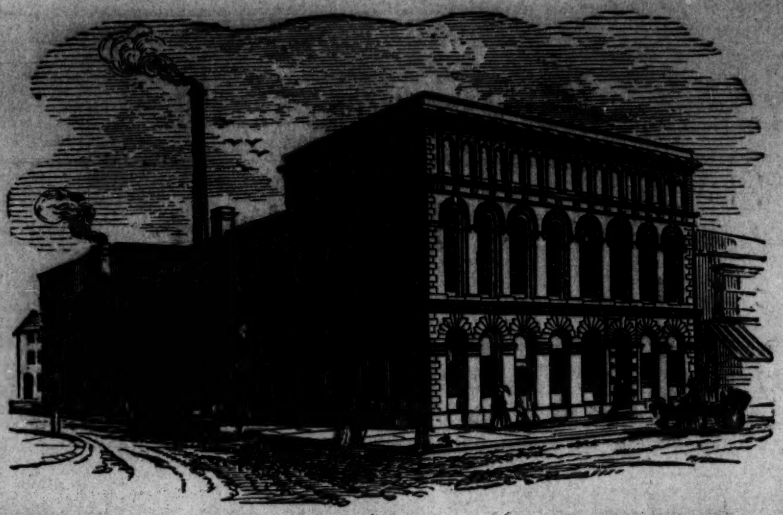

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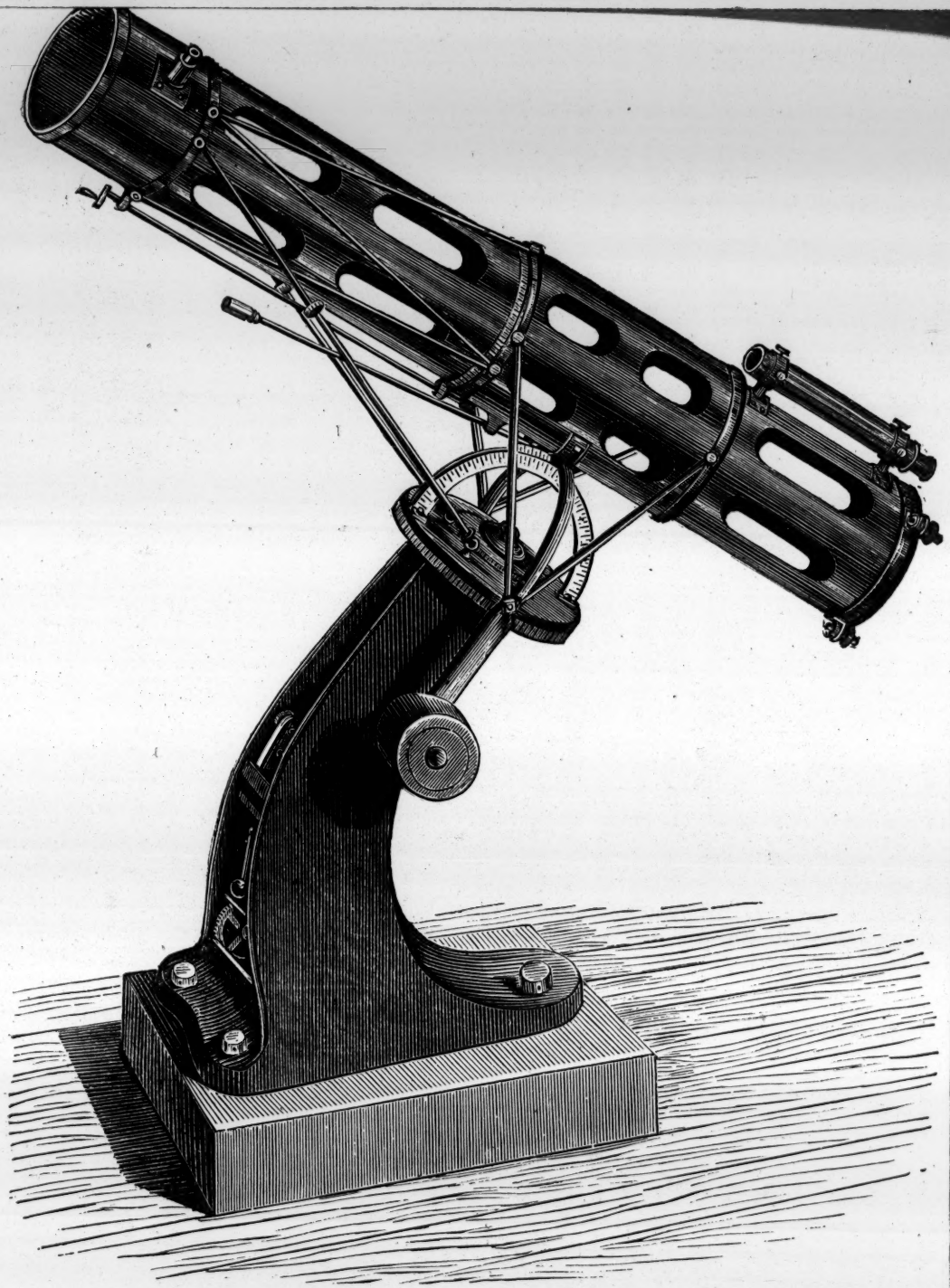

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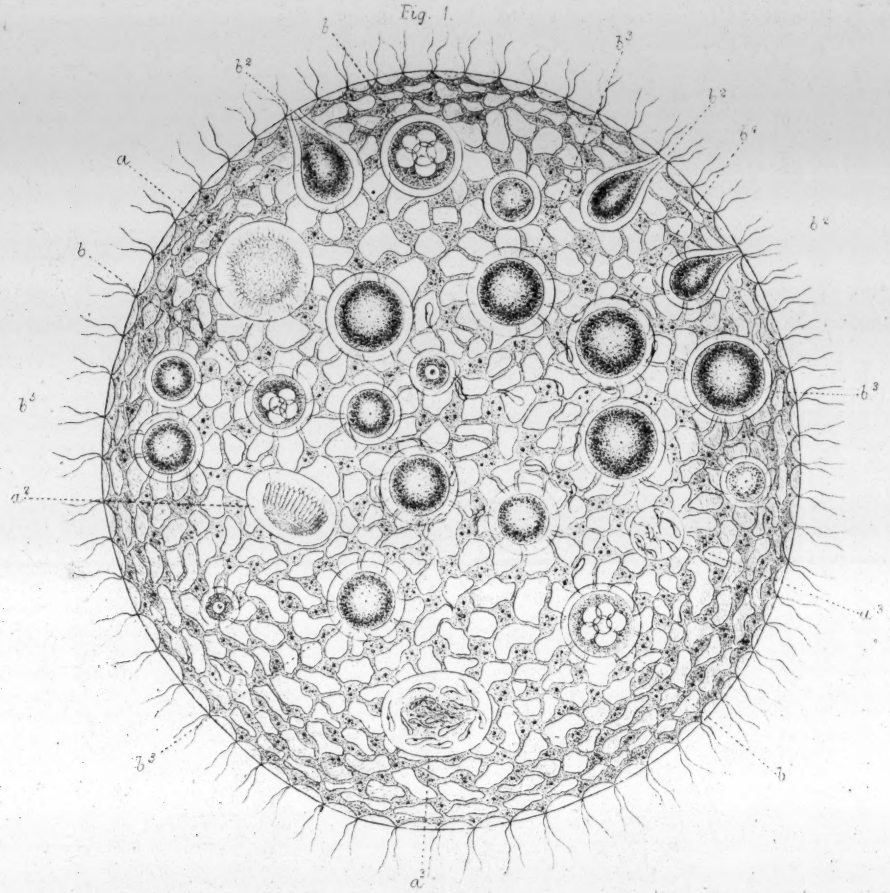

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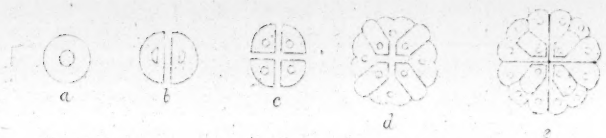

Fig. 5.

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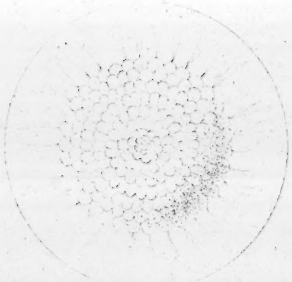


Fig. 6.

Fig. 7.

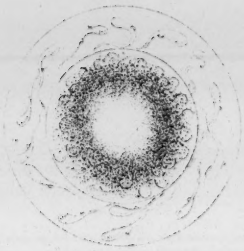


Fig. 8.

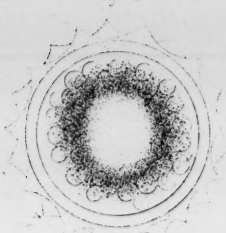


Fig. 3.

Volvox Globator (after Cohn.)

Mintern Bros lith.

VOLVOX GLOBATOR.

By ALFRED W. BENNETT, M.A., B.Sc., F.L.S.

LECTURER ON BOTANY AT ST. THOMAS'S HOSPITAL.

[PLATE V.]

THE organism which we are about to describe has long been a favourite object for observation to microscopists, not only from the extraordinary beauty and delicacy of its structure, but from the remarkable phenomenon which it presents of a member of the vegetable kingdom endowed with a power of motion apparently as spontaneous as that possessed by any organisms belonging to the animal kingdom. The very location of the *Volvox* among plants has indeed been challenged. Like many others which are now placed, by almost universal consent, among the lower classes of Cryptogams, it was held by the distinguished naturalist Ehrenberg to be of animal nature. If, however, any doubt remained as to its true vegetable character, this has been completely set at rest by the researches of Professor Cohn,* who has only quite recently for the first time followed out completely the various stages of reproduction of this singular organism. In no English work will they at present be found adequately described, although so many observers had before his time had it under their notice.

The *Volvox globator* is not uncommon in clear pools on commons, and in similar situations, and is visible to the naked eye as a minute pale-green globule rolling through the water, passing rapidly across the field of view under the microscope, its full diameter being perhaps $\frac{1}{50}$ of an inch. Under the microscope it is seen that the rotatory motion is caused by innumerable fine transparent vibratile cilia, with which the surface is studded. The appearance presented by the entire body is that of a globular transparent, hollow, membranous sac, through which these cilia protrude, and studded with bright green points, while in the interior are seen a small number of dense green globes. In the autumn or early winter these internal globes have often

* "Beiträge zur Biologie der Pflanzen," Vol. i. Heft 3, 1875.

assumed an orange colour, giving to the whole globule a red tinge. The vibratile cilia already described are arranged in pairs, each pair belonging to a separate peripheral corpuscle or cell. These peripheral cells contain each a green protoplasm-body, varying in shape according to the age of the individual, as shown in fig. 2, *a*, *b*; they usually contain a minute starch-granule, a reddish-brown "eye-spot," analogous to that of swarm-cells (zoospores) and of the Flagellate Infusoria, and one or two contractile vacuoles, the cilia being borne at the narrow hyaline end. Each is surrounded by a gelatinous envelope, which is pierced by a number of canals, all lying nearly in one plane, and filled by green or colourless extensions of the proto-plasmic interior. Since the canals of adjoining cells correspond, the corpuscles appear as if connected together by a network of fine reticulations. The outer gelatinous wall of each cell is also perforated by two pores, through which the two vibratile cilia protrude into the surrounding water. They constitute a single peripheral layer enveloping the entire organism, corresponding to the plate which is characteristic of many Chroococcaceæ; each one by itself would be undistinguishable from an ordinary swarm-cell of many filamentous Algæ, or from the entire individual of *Chlamydococcus* or *Pleurococcus* in its motile stage. They have, however, so far as is known, no reproductive function, and in this respect stand almost alone among cells endowed with a spontaneous power of motion.

Besides these non-reproductive or sterile cells, there are in each *Volvox*-colony three kinds of reproductive cells—non-sexual (neuter), male, and female. The neuter reproductive cells, or parthenogonidia, as Cohn terms them, are similar in structure to the sterile cells, but two or three times their size, *i.e.* from ·006 to ·009 mm. in diameter. Very early in the development of the young colony, they begin to multiply by bipartition, and all the cells in the same colony or sphere are usually at one time in the same stage of development. This division has been followed by Cohn through four stages, as shown in fig. 3, *a–e*, the fourth stage consisting of sixteen cells, beyond which he was unable to trace it, owing to the difficulty of the observation. At this stage it is described as having somewhat the form of a blackberry, each segment possessing a single chlorophyll-granule containing starch. The young colony is surrounded by a transparent membrane, which it at length breaks through, and carries on an independent existence within the cavity of the mother-colony, each of its cells developing a pair of cilia; finally it escapes from its confinement in the mother-colony into the surrounding water. The normal number of parthenogonidia which thus develope into colonies within the mother-colony is eight, corresponding to the eight cells into which each of them

breaks up on its third segmentation. The young colonies complete their growth in a few days, attaining a diameter of from 00·10 to 00·15 mm., and have by this time absorbed the greater part of the chlorophyll and starch of the mother-colony.

The sexual reproductive cells, male and female, are very few in proportion to the sterile cells. While the non-sexual reproduction by parthenogonidia takes place during the whole year, the sexual cells are apparently formed only in the autumn. The two kinds are found either in the same or in different colonies, establishing the classification of the genus into monœcious and diœcious sub-species, as first pointed out by Cohn. Fig. 1 represents a monœcious colony. The two modes of reproduction do not take place simultaneously in the same colony, the sexual generation forming the close of a longer or shorter series of non-sexual generations. *Volvox* presents, in fact, one of the earliest instances in the ascending series of the phenomenon known as alternation of generations, which attains its highest development in plants in the Vascular Cryptogams. The female cells (gynogonidia of Cohn) are at first undistinguishable from the parthenogonidia, but are much more numerous. On their first appearance they are about three times the size of the sterile cells; their protoplasm-body increases rapidly, and becomes of a dark green colour from abundance of chlorophyll. They have at first a frothy appearance (fig. 1, *b*) from the formation of vacuoles, but afterwards appear to be filled with protoplasm; and they are now at once distinguishable from the parthenogonidia by their never dividing. They soon become flask-shaped (fig. 1, b^2), their narrow end touching the periphery of the sphere, and the larger end hanging free into the cavity. When ready for impregnation they round themselves off into a spherical form (fig. 1, b^3), and may then be designated oospheres, each being enveloped in a gelatinous membrane or oogonium. The male cells, or androgonidia, present at first a still closer resemblance to the parthenogonidia, since, when they have attained about three times the size of the sterile cells, they begin to divide; but they are of a lighter colour, from containing a smaller quantity of chlorophyll. The divisions also take place only in two instead of in three directions, thus developing, not into a sphere, but into a plate of cells. They ultimately resolve themselves into a bundle of naked primordial cells (fig. 1, *a*), each consisting of a thicker but elongated body, in which the chlorophyll has been transformed into a reddish yellow pigment, and of a long colourless beak, to the base of which are attached two very long vibratile cilia, and where also is a red corpuscle or eye-spot (see figs. 4, 5, 6). The whole androgonidium may now be considered as an antheridium enclosed in a gelatinous envelope, each of the naked protoplasmic bodies being a motile antherozoid or spermatozoid. About the same

time that the gynogonidia have matured into oogonia, and their protoplasmic contents into oospheres, the movements of the vibratile cilia attached to the antherozoids begin to set the whole antheridium in motion, in the positions indicated in fig. 1, *a*, a^2; the antheridium soon breaks up, and the separate antherozoids are seen in rapid independent motion within the gelatinous envelope of the antheridium (fig. 1, a^3), which they ultimately break through, and then move about rapidly in all directions within the cavity of the mother-colony. Their movements are described by Cohn as being remarkably beautiful, the constant curvings of the elongated neck of the antherozoid being compared by him to those of the neck of a swan. They assemble in large numbers round the oogonia, and some of them finally penetrate through their gelatinous envelopes (fig. 1, b^4, fig. 7), and coalesce with their protoplasmic contents or oospheres. The fertilised oosphere is now an oospore, and developes a new cell-wall, the epispore, which is at first smooth, but afterwards covered with conical elevations, giving a section of it a stellate appearance (fig. 8). A second perfectly smooth membrane, the endospore, is subsequently formed within the first. The chlorophyll gradually disappears, and is replaced by an orange-red pigment dissolved in oil, so that the mature oospore, while still enclosed within the mother-colony, is of a bright red colour, giving the red tinge which *Volvox* often presents, even to the naked eye, at certain periods of the year. Individuals containing oospores still enveloped in their spiny coatings were described by Ehrenberg as a distinct species, under the name of *V. stellatus*. The number of sexual reproductive cells in a colony of *Volvox* varies greatly; Cohn has observed five or more male and about forty female cells.

Soon after the oospore reaches maturity, the mother-colony breaks up, single cells escaping from the combination and swimming about freely in the water; their further history is unknown; while the oospores fall to the bottom and there hibernate. Their further development has only been observed by Cienkowski, who states that the contents of each spore break up into eight spheres which ultimately swarm out.

Ehrenberg described two species of *Volvox*, *V. globator* and *V. stellatus*, both of which he considered to be infusorial animalcules. These have been determined by Cohn to be different stages of the same species, the former in its parthenogenetic, the latter in its sexually reproductive condition; Ehrenberg's *Sphærosira Volvox* is also another form of the same species. His *Volvox aureus*, and Stein's *V. minor*, are considered by Cohn to form a distinct sub-species of *V. globator*, differing from the typical form in the colonies being of a smaller size, and dicœcious instead of moncœcious, and in the epispore of the

oospore being smooth. The mode of fertilization of the latter form has not yet been directly observed. The genus is cosmopolitan, having been found throughout Europe, in North Africa, India, and North America.

The old family of Volvocineæ, which included the genera *Volvox*, *Eudorina*, *Pandorina*, *Gonium*, and *Stephanosphæra*, is placed by Sachs, in the 4th edition of his "Lehrbuch," under the Zygosporeæ, and in the section characterized by the conjugation of zoogonidia (swarm-spores or zoospores). It is evident, however, that the family, as so constituted, cannot be maintained, and Rostafinski's classification must be accepted as preferable, retaining those genera which exhibit the conjugation of swarm-spores as a family of Zygosporeæ under the name of Pandorineæ, while the true Volvocineæ are placed in the class of Oosporeæ,* in which the process of impregnation consists in the coalescence of one or more minute spontaneously motile antherozoids which escape from an antheridium, with a comparatively large oosphere contained in an oogonium. The process indeed bears a very close analogy to the mode of fertilization in one of the highest families of Oosporeæ, the Fucaceæ. The family Volvocineæ, as thus constituted, includes at present only the two genera *Volvox* and *Eudorina*. The structure and developmental history of the latter have been described by Carter.†

* This is the course followed in M'Nab's recent class-book of botanical classification.

† "Annals of Natural History," 3rd series, Vol. ii. 1858, p. 237.

EXPLANATION OF PLATE V.

Fig. 1. A monœcious colony of *Volvox globator*, *a*, a^2, a^3, antheridia; *b*, b^2, gynogonidia; b^3, b^4, oospheres.

Fig. 2. Ciliated peripheral cells.

Fig. 3. *a-e* Parthenogonidia in successive stages of division.

Fig. 4. Complete antheridium.

Figs. 5 and 6. Antherozoids.

Fig. 7. Oosphere containing antherozoids (see also fig. 1, b^4).

Fig. 8. Fertilized oosphere.

All the figures highly magnified.

ON SOME RECENT CONTRIBUTIONS TO THE THEORY OF THE MOTION OF THE MOON.

By E. NEISON, F.R.A.S.

A QUARTER of a century has now elapsed since the late Professor Hansen, of Seeberg, near Gotha, completed his elaborate researches on the motion of the moon, and he was enabled to announce that the lunar theory as developed by him fully represented the motion of the moon during the preceding century 1750–1850. Professor Hansen had then been at work on this recondite subject for nearly thirty years, and had won a high reputation, not only from the ingenious and powerful methods he had devised for attacking the most difficult problems of gravitational astronomy, but also from the entire mastery over the minutest details of the lunar theory which he had shown by his discovery of two inequalities of long period in the motion of the moon due to the attraction of the planet Venus. This announcement of Professor Hansen's coming from so high an authority was most gratifying to astronomers, for it showed that after one hundred years' work science had at last gained complete mastery over one of the most difficult problems of astronomy.

The discovery by Professor Hansen of the existence of these two terms of long period was a step of the greatest importance, for it appeared to completely remove the last great difficulty in the lunar theory, a difficulty which had been puzzling astronomers for many years; and it has been termed by Sir G. Airy "one of the greatest steps—perhaps the greatest step—in gravitational astronomy." For years it had been known that the moon was at times before, and at other times behind, the place in its orbit which it should have theoretically occupied. It had further been shown that this alternate retardation and acceleration was periodical in its character, and that it required about ninety years to proceed from one extreme to the other. This difference, moreover, was not unimportant, for at one time the moon would reach any fixed point in its orbit more than half a minute before its theoretical time, and at some other period it would be over half a minute later than the time fixed by theory. And what rendered this inequality the more perplexing to astronomers was the fact that they could not imagine any source from which it was even possible that there could arise such an

inequality. That it did not arise from the attraction of the sun appeared to be certain from the investigations of Laplace, Plana, Poisson, and Pontécoulant; that so large an inequality as this was due to the attraction of the planets seemed to be out of the question; and it did not appear possible that it could be due to the attraction of the earth. It was left for Professor Hansen to discover its origin in an unlooked for source, namely, in the attraction of the planet Venus. He showed that this planet, from its attraction on the earth and its satellite, produced two terms of long period in the motion of the moon; by a long and laborious calculation he computed the value of these terms; and he showed that the combined effect of these two terms of long period exactly accounted for that observed inequality in the motion of the moon which had so long perplexed astronomers.

The first of the two new terms in the motion of the moon which were discovered by Professor Hansen was principally due to the direct attraction of Venus upon the moon. From a remarkable numerical relation between the motion of the moon in its orbit and the motion of Venus and the earth around the sun, for one hundred and forty years at a time the action of Venus on the moon tends to increase its distance from the earth and so render its motion around the earth slower, whilst for the next one hundred and forty years exactly the reverse action takes place. Thus from the direct attraction of Venus on the moon, our satellite moves slower round the earth for one hundred and forty years, so that at the end of this period it is behind its mean place; and then for the next one hundred and forty years it moves quicker round the earth, and gets correspondingly before its mean place. This may be called the direct term of long period. The second term of long period discovered by Professor Hansen arises from the indirect action of Venus upon the moon. Sir G. Airy had discovered in 1828 that from the attraction of Venus upon the earth, this last body would alternately move in a rather larger or smaller orbit than it would have were it not for the attraction of Venus. He showed that for one hundred and twenty years the earth would be rather more distant from the sun, and for the next one hundred and twenty years, rather less distant from the sun, than would be otherwise the case. Professor Hansen now showed that this would cause a sensible variation in the motion of the moon. From the disturbing action exercised by the attraction of the sun, the moon cannot move round the earth so quickly as it would otherwise do. During then the term of one hundred and twenty years, when the earth had its distance from the sun increased from the attraction of Venus, the action of the sun is decreased, and the moon is enabled to move more quickly round

the earth, and at the end of the one hundred and twenty years has gained an appreciable amount, whilst for the one hundred and twenty years when the earth is nearer the sun the reverse phenomenon takes place. This term may be called the indirect term of long period. The importance of these terms arises from their effects being cumulative for so many years, that although very small, at the end of the long period in which they act in one way they accumulate to something considerable.

It was generally considered by astronomers that Professor Hansen had succeeded by this means in removing the last difficulty in the theory of the moon, and Sir John Herschel remarked in his address to the Royal Astronomical Society, "that their discovery may be considered as a practical completion of the lunar theory, at least for the present astronomical age, and as establishing the entire dominion of the Newtonian theory and its analytical applications over that refractory satellite." This view received full confirmation when it was found that Hansen's "Tables de la Lune," based on his investigation, accurately represented the motion of the moon. It appeared certain that the difficulties of the lunar theory were at last mastered.

Unfortunately this sanguine view has been found to be premature; unexpected difficulties have sprung up in the lunar theory, and Hansen's "Tables de la Lune," which so well represented the place of the moon during the interval 1750–1850, have become seriously inaccurate, and are rapidly becoming more and more erroneous. It is certain, therefore, that there is still some serious deficiency in the theory of the moon which formed the basis of Hansen's "Tables de la Lune." It is therefore a matter of the greatest importance that the lunar theory should be rendered more perfect and the tables of the moon made more accurate. The practical importance of this is so great that it is probable that before long steps will be taken by the authorities of both the American and English Nautical Almanacs to empirically correct the tables, so as to render the tabular places more accurate.

It is now generally known that Hansen, to make his tables represent the motion of the moon during the interval 1750–1850, was obliged to give empirical values to the coefficients of the two terms of long period which he had discovered to be produced by the action of Venus. Hansen considered he was justified in doing this by the circumstance that these empirical values enabled his tables to represent with accuracy the motion of the moon during the century prior to 1850, for it appeared most improbable that the tables could represent the motion of the moon for a whole century and yet fail to continue to represent it. The tables, however, did not continue to represent the

motion of the moon, and have been steadily deviating from the truth, until at the present time they show most serious and increasing errors. It is, therefore, certain that at least one, if not both of the empirical values which Hansen assigned to these two terms of long period must be erroneous. What then are the correct values, as Hansen's are wrong? Many of the recent contributions to the lunar theory have implicitly turned on this point.

The late M. Delaunay was the first to take up this subject, for he was unsatisfied with the manner in which Professor Hansen had dealt with these terms of long period. Delaunay had then been engaged for ten years on the theory of the moon, and he had devised an entirely original method of attacking the problem. This new method which he employed was most elegant and ingenious. Instead of attacking the problem all at once, he showed that it was possible to divide it into several hundred simpler problems, and to solve each of these separately. Each of the many hundred different perturbations to which the motion of the moon is subject was taken by itself and its influence separately calculated. By this means he had to deal with over a thousand simple operations, instead of two or three dozen much more complex operations. This new method is in practice very laborious, it is very liable to errors, and it requires much skill and experience in its use, but its analytical elegance and advantages won a high reputation for M. Delaunay. Moreover, in his skilled hands it proved a most successful means of attacking the problem, for he was enabled by its means to carry the lunar theory to a degree of perfection far beyond any of his predecessors.

Delaunay applied this method to the determination of the values of the two terms of long period which had been discovered by Hansen to be produced by the attraction of the planet Venus. He considered, first, the one which is due to the direct action of Venus on the moon; and in his calculations he neglected the effect of the small inclination of the orbit of Venus, because he considered that it would not be of any consequence. He found that this first term of long period would be very small, and he announced that Hansen's value was more than fifty times too large. He was induced, however, to re-examine his investigations, and, to render them somewhat more complete, he took into account the small inclination of the orbit of Venus. This new result he found very different from the previous, for it agreed closely with the value found by Hansen. The small inclination of the orbit of Venus which he had thought to be of no importance proved to be the very cause why the attraction of Venus was able to so much disturb the motion of the moon. The value for this term of long period which Delaunay had thus

calculated was found to agree well with the value empirically assigned to it by Hansen, so that this part of Hansen's tables was confirmed. It is true it differed from the complete theoretical value which Hansen had found for this term, but this was of slighter moment, for Hansen himself had expressed a strong doubt of its accuracy.

Delaunay proceeded next to the determination of the value of the second term of long period, namely, that which results from the indirect action of Venus. He calculated the value of this by the same method as before, but taking care to consider the effect of the inclination of the orbit of Venus, he announced that he made the value of this term less than one-hundredth of that assigned to it by Hansen. This was a most important matter, for if Delaunay was right in his conclusion, the tables of the moon calculated by Hansen must be seriously inaccurate, for without this term which Delaunay declared was so greatly in error, these tables would not represent the motion of the moon with any accuracy at all. At this period, however, the tables of the moon did represent its motion with considerable accuracy, which appeared to be a strong indication that Hansen was right, and not Delaunay. Moreover, it has been mentioned that Delaunay employed his new method of attacking the lunar theory in determining the value of these inequalities, and this rendered it difficult for those unfamiliar with it to follow his calculations. It was only when Hansen's tables of the moon were seen to be developing serious errors that general attention was turned to Delaunay's investigations.

Professor Newcomb, of Washington, was the next to take up the subject, which he in turn examined by a method of considerable novelty, but he has not published the details of his investigations. He was able, however, to completely confirm the results which had been arrived at by Delaunay. It appeared certain, therefore, that Hansen's tables were wrong. By this time, however, this last fact was unquestionable, for the tables were rapidly deviating from the real place of the moon, and it was obvious that there was some grave imperfection in Hansen's work.

In 1873 Sir G. Airy pointed out that the theoretical investigations of Delaunay and Newcomb appeared to render it certain that Hansen's second term of long period, namely, that mainly due to the indirect action of Venus, had no right to be present in his tables. He therefore directed his attention to determining whether it was possible after removing this term to make the tables represent the motion of the moon by suitably correcting certain of the elements of its orbit. Sir G. Airy found, however, that this was not possible. Even after applying the best possible corrections, there remained outstanding errors

of such magnitude that he felt convinced that there must still exist some serious defect in the lunar theory as developed in the theoretical basis of the tables.

Professor Newcomb has lately completed an elaborate investigation of the earlier observations of the moon. He takes for his data a number of eclipses of the moon, which are quoted by Ptolemy in his "Almagest," a still larger number which were observed by the Arabians during the ninth and tenth centuries, and a number of observations of eclipses and occultations which were made by different astronomers during the seventeenth and early part of the eighteenth centuries. The greater number of these last have remained unpublished since the time they were made, and were discovered by Professor Newcomb when examining the archives of the Paris observatory. These last constitute the basis of a most important contribution to astronomy, for by carefully reducing them Professor Newcomb has been enabled to deduce with considerable accuracy the observed place of the moon for epochs much anterior to 1750, where previously had terminated our exact observations of the moon. In fact, by means of this investigation of Professor Newcomb, astronomers can trace back the observed path of the moon with some exactitude to 1675, and with less certainty to 1620. Professor Newcomb has made use of the data furnished him by this investigation for the purpose of ascertaining how far the observed place of the moon agrees with Hansen's tables for the period anterior to 1750. He shows that they fail to do so, and that for epochs prior to 1750 Hansen's tables are as discordant with observation as they have proved to be for epochs subsequent to 1850. Assuming with the Astronomer Royal that one of Hansen's terms of long period ought to be rejected, Professor Newcomb then endeavours to ascertain what corrections must be introduced to make the tables best represent the observations. He shows that the mean motion of the moon must be decreased by nearly thirty seconds in a century, and the value of the secular acceleration of the mean motion be decreased by nearly one-third of the amount assigned to it by Hansen. Even, however, when the tables are thus corrected in the best possible manner, there remain such large outstanding errors that Professor Newcomb comes to the same conclusion as Sir G. Airy, that there is still some serious defect in the lunar theory.

Professor Newcomb draws attention to the fact that these outstanding errors can be entirely accounted for by supposing that the earth is subject to periodical inequalities in its rotation on its axis. For some years it had seemed to Professor Newcomb that this was the most probable hypothesis on which to explain these deviations between theory and observation. As he points out, it would be a most serious and unwelcome one, for, granting

its truth, it would be no longer possible to predict the apparent motion of the moon, since the changes in the rotation of the earth could not be expected to follow any determinate law. Further investigations have led Professor Newcomb to the gratifying conclusion that we may yet hope to find the source of these deviations in the effects of the attraction of some member of the solar system. Closer examination of the outstanding errors showed Professor Newcomb that they could be explained by a new periodical term with a period of about 260 years. This is very nearly the period of the term of long period which was discovered by Professor Hansen to be produced by the direct action of Venus on the moon. It occurred to Professor Newcomb that by slightly changing the constants in this term the outstanding errors might be reduced. The result was most striking. This term, like the other inequality in the motion of the moon, has for its argument a quantity proportional to the time added to a constant quantity; and Professor Newcomb found that merely by changing the sign of this constant part of the argument, all the considerable outstanding errors disappeared. It is, in fact, merely necessary to make this slight change, and Hansen's tables, when corrected as previously mentioned, well represent the motion of the moon from 1625 to 1875. This led Professor Newcomb to examine whether there was not some accidental mistake in the sign of this constant quantity, but he found that it was perfectly correct. It is, therefore, a mere chance that this change of sign of this quantity causes the outstanding errors to disappear in this way.

These investigations of Professor Newcomb very clearly establish the following conclusions. Firstly, that Hansen was entirely wrong in the value which he empirically assigned to one, at least, of the terms of long period in the motion of the moon, which arose from the attractions of Venus. Secondly, that if Delaunay's value of these terms be correct, there still remains to be discovered at least one more term of long period in the motion of the moon. Thirdly, that if both Hansen's tabular value and Delaunay's theoretical value be wrong, it is possible to satisfy the observations without supposing any new term to exist.

Quite recently another contribution has been made to this subject, which places the theory of these terms of long period in an entirely fresh light, and opens a means of reconciling the contradictory results obtained by two such distinguished astronomers as Hansen and Delaunay. Hansen never published an account of the manner in which he theoretically calculated the complete value of these terms of long period, but finding two results arrived at by different methods to be irreconcilable, he postponed publishing his investigations until he had completed

a third calculation. It is understood that this third investigation was never completed. It is well known, however, that Hansen would not admit the correctness of Delaunay's results, although he never stated his reasons for considering them to be imperfect. Little importance, however, seems to have been attached to Hansen's refusal to admit that Delaunay's researches invalidated his own, because it seemed natural to suppose that had Delaunay's investigations been imperfect, Hansen would have at once pointed this out. It has always seemed to me that Hansen must have had some very good reason for his objection to Delaunay's results, but that he was waiting until he had completed his third investigation of this difficult problem. Moreover it has already been mentioned that Delaunay employed his own peculiar method of attacking the problem, so that one who was not perfectly familiar with this new method would not readily follow the research. The last contribution to this subject opens the question—are Delaunay's investigations complete? This is a most important matter, for if Delaunay has omitted to take into consideration the whole of the problem, it is easy to see not only why there should be a difference between Hansen's and Delaunay's results, but why Hansen refused to admit the accuracy of Delaunay.

In his investigations Delaunay has very carefully calculated the effect of the attraction of Venus on the moon, and the effect of Venus upon the earth, and in this way indirect effect on the moon. The manner in which the planet thus produces inequalities in the motion of the moon has been already detailed. In these calculations Delaunay has carefully taken into consideration the fact that, owing to the difference in the attraction of the sun upon the moon and earth, the moon in its motion around the earth is affected by numerous inequalities. In all this work Delaunay's results are perfectly accurate. Beyond this Delaunay has not gone, and this really comprises the entire direct way in which Venus influences the motion of the moon. It has also been shown that from this action of the planet Venus for over a hundred years at a time, the moon moves in a very slightly larger orbit than it could were there no Venus to attract it, and that this is then followed by a similar period in which the moon moves in a proportionately smaller orbit. What Delaunay has not taken into consideration is the way in which the action of the sun upon the moon will be affected by this alternate enlargement and contraction of the orbit of the moon arising from the action of Venus. It is known that the slower the moon revolves around the earth, the more powerful is the disturbing action of the sun and the greater is the power it has of enlarging the orbit of the moon. During the period, therefore, when the attraction of the planet Venus

has enlarged the orbit of the moon, this body will move slower around the earth, the sun will have greater power over it and will be able to still further enlarge the orbit and cause the moon to revolve still slower around the earth. As this action will continue to accumulate for over one hundred years at a time, at the end of this period it may amount to a considerable retardation of the moon. From this effect of the attraction of the sun on the effect produced by the attraction of Venus, exactly similar inequalities will be produced to those arising from the action of the planet alone. From certain expressions on the part of Hansen it seems that he has not failed to take these circumstances into account, whilst Delaunay has entirely omitted to consider them in his investigations, so that his researches must be held to be imperfect. It is evident, therefore, that further research will be required to satisfactorily determine the value of these two terms of long period, and to decide whether Delaunay or Hansen is correct, or whether either is. And until this is done the theory of the terms of long period in the motion of the moon must be held to be in an unsatisfactory condition.

It is well known that the mean motion of the moon is at present slowly increasing, so that there is a gradual acceleration in the velocity with which it moves around the earth. This secular acceleration was discovered by Halley, a celebrated English astronomer, and for many years the greatest mathematicians were unable to discover its cause. In 1787, however, Laplace discovered that from the slow change in the eccentricity of the orbit of the earth, the mean motion of the moon would have a gradual secular acceleration, and on calculating what it would be, he found it would amount to rather over 10″ in a century. Now this was exactly the amount of the observed acceleration which had been discovered by Halley. Laplace's discovery appeared, therefore, to completely account for this observed acceleration of the velocity of the moon's revolution around the earth. Twenty-five years ago Professor Adams discovered that when the calculations were carried to a higher order of accuracy than had been considered necessary by Laplace, it very much reduced the value of the secular acceleration due to the decrease in the eccentricity of the orbit of the earth. At the time this discovery of Professor Adams gave rise to a very animated controversy, but it is now certain that the views of Professor Adams are perfectly correct, and that the secular acceleration in the mean motion of the moon which is due to the decrease in the eccentricity of the orbit of the earth, does not amount to more than 6·2″ in a century.

Professor Hansen, when he compared his tables with the ancient observations, found that they indicated a value of the

secular acceleration of 12·2″, or nearly double that which has received a theoretical explanation. This leaves a difference of 6″ per century in the secular acceleration whose origin had to be found. Newcomb and Delaunay both suggested that perhaps this difference between the observed and theoretical values of the secular acceleration might be only apparent, and that the difference might be due to a gradual increase in the length of the day. It was pointed out that if the earth be supposed to be slowly retarded in its axial rotation, the day will be longer, and as the moon will advance further in its orbit in the space of one day, it will appear to have a secular acceleration. An increase in the length of the day of only one thousandth part of a second in a century would amply account for the difference between the theoretical and observed secular acceleration. It remained to be shown what would produce this retardation of the earth's axial rotation. Both Newcomb and Delaunay suggested that it might arise from the action of the tides, which, by their friction on the solid crust, might prove to be the origin of this decrease in the velocity of the rotation of the earth. Delaunay attempts to show, mathematically, that it was possible for the tides to produce this effect. It cannot, however, be said that this attempt was successful.

This assumed retardation of the axial rotation of the earth and its hypothetical origin in the friction of the tides of the ocean against the earth, have been very generally accepted, and are very commonly employed for purposes of scientific argument, as if their truth had been rendered highly probable. It is, however, important to remember that it has never yet been shown in a satisfactory manner that it is even possible for the tides to retard the rotation of the earth to a sensible extent. Moreover, as will now be shown, it is still most uncertain that there is really any retardation to be accounted for in this way. It must not be forgotten that the value found by Hansen presupposes that his tables are correct, but it has been seen already that this is not the case. The late investigations of Professor Newcomb afford much valuable information on this subject.

It has been already mentioned that Professor Newcomb, when correcting the data employed in Hansen's tables, found that the value of the secular acceleration which had been employed ought to be diminished by one-third. According to him the value 8·4″ is that which best represents the observations, and these observations are more trustworthy than any previously made use of. Thus it is evident that the greater part of the supposed difference between the values of the theoretical and observed secular inequality arises from the imperfect nature of the observations which hitherto have been made use of, together with the effect of the empirical term employed by Hansen to

bring his tables into accord with observation. In determining the correction to the secular acceleration employed by Hansen, it would appear that Professor Newcomb has attached considerable weight to the very earliest set of observations in his list. It is almost certain, however, that there is some serious imperfection in this set of observations, for they are entirely discordant with the remainder of the early observations, which are tolerably consistent with each other. As it is, Professor Newcomb has been unable to make the corrected tables represent this set of observations within nearly 16′, whilst it very materially interferes with the other observations. For this reason this earliest set of observations may have proportionately less weight attached to it.

Under these conditions it will be found that without materially interfering with the accuracy with which the modern observations (1625–1875) are represented, the ancient observations, including both the Arabian and the observations quoted by Ptolemy, can be much better represented by still further decreasing the value of the secular acceleration employed by Hansen. In fact, the value which best accords with the whole series of observations is 7·2″, or within 1″ of the theoretical value of the secular inequality. This reduces the supposed retardation of the rotation of the earth to a very slender foundation.

Professor Newcomb points out the very important fact that the more recent observations indicate a value for the secular acceleration much less than that indicated by the earlier ones. In fact, the Arabian observations are best represented by the theoretical value of the secular acceleration, which is 6·2″, while the ancient observations quoted by Ptolemy require the value 7·4″. This would seem to point to the existence of some periodical inequality. For some time it has appeared to me that the observations indicated the existence of a large term of very long period in the mean motion of the moon. This view is now confirmed by these investigations of Professor Newcomb. Such a term would require to have a period of over two thousand years to have a sensible effect on the secular acceleration. I have already shown that terms of this long period do exist in the mean motion of the moon. It remains to show that one of these terms has a coefficient of sufficient magnitude. Assuming the existence of such a term, and it can be shown that it would not only account for the difference between the values of the secular acceleration indicated by the modern and ancient observations, but it would remove the remaining difference between the observed and theoretical values of the secular acceleration. For this purpose the term would have to have a period of about 3,600 years, and a coefficient of under two minutes of arc.

Then correcting Hansen's terms of long period in the manner suggested by Professor Newcomb, diminishing Hansen's value of the mean motion by about forty seconds per century, and employing the theoretical value of the secular acceleration, the modified tables will completely represent the observations both ancient and modern. Indeed, by slightly altering the period of the term of long period, its coefficient might be much reduced, and it would still represent, with tolerable accuracy, the observations.

These considerations show that recent investigations go far to remove the supposed discrepancy between the value of the secular acceleration in the mean motion of the moon which is given by theory and by observation. They are sufficient to show that there is no necessity to assume any retardation in the rotation of the earth due to friction produced by the tides of the ocean.

In connection with the American expeditions for observing the transit of Venus, it became necessary to examine the corrections to Hansen's tables of the moon given by the observations made during the period 1862–1874. After applying various slight corrections to the differences between the observed and tabular places of the moon, Professor Newcomb was surprised to find systematic errors outstanding which could not be represented by corrections to the elements of the lunar orbit. At first he was inclined to attribute them to irregularities on the surface of the moon, but a more careful examination showed that they were periodic. Further investigation showed Professor Newcomb that they arose from small variation in the eccentricity and longitude of the perigee of the lunar orbit, this variation going through all its changes in a period of nearly seventeen years. Owing to these variations the moon would be periodically accelerated or retarded in its orbital motion by about three seconds of time. Professor Newcomb was unable to offer any explanation of the origin of these periodical variations. It has since been shown that they arise from the attraction of Jupiter on the moon in its revolution around the earth.

From the attraction of the sun upon the moon in its revolution around the earth, a great inequality is produced in the motion of the moon, from which its position in its orbit is alternately increased or decreased by nearly three hours. This great inequality was discovered by Ptolemy, and was subsequently named the *evection*. Every planet, by its attraction, produces a similar term, although much smaller; so much smaller, indeed, that it was generally thought that they would be too small to be ever detected. In general, this view is perfectly correct, and it would also be the case with Jupiter were

it not for the fact that Jupiter and the perigee of the lunar orbit have nearly the same period of revolution. Owing to this, the influence of Jupiter on the motion of the moon is very much augmented. It has also been discovered that Jupiter produced a still larger inequality in the motion of the moon, one twice as great as that discovered by Professor Newcomb, and having a period of more than seventeen years. This new inequality also derives its magnitude from the commensurability in the periods of revolution of Jupiter and the perigee of the lunar orbit, as from this reason the attraction of Jupiter continues to accumulate for nearly nine years at a time, amounting, at the end of this period, to an effect of appreciable magnitude.

A most able paper has been recently published by Mr. C. W. Hill, a mathematician connected with the American Nautical Almanac, in which he calculates the portion of the motion of the lunar perigee which is independent of the eccentricity and inclinations of the orbits of the sun and moon. The theoretical determination of the motion of the lunar perigee has always been a task of considerable difficulty, so that even the best strictly theoretical value of the period of revolution of the perigee around the lunar orbit differs from the observed value by about five minutes. This is a very considerable amount in the present advanced condition of the lunar theory, seeing that theory gives within a few seconds the same value as the observed period of the revolution of the line of nodes of the lunar orbit. Mr. Hill devised an entirely new method of determining the motion of the lunar perigee by skilfully utilizing some of the later developments in mathematical analysis. In this manner a very accurate value for the motion of the lunar perigee was obtained. In England, Professor Adams had independently arrived at the same method of solving this problem, and he had moreover determined the rate of motion of the line of nodes in the same manner. In these papers a most important contribution has been made to the lunar theory, for they open a means of determining the values of the inequalities in the motion of the moon which promises to be of great value. Its intricacy, and the great labour it involves, will probably however, restrict, its use to special cases.

CLOUD OUTLINES.

Second Paper—Forms of Cumulus.

By Rev. SAMUEL BARBER, F.M.S.

In a former article in the *Popular Science Review*, attention was drawn to the importance of a close examination of the outlines of the various forms of cloud, and the instances selected for observation were the "electric cumulus," the "anvil" cloud, and the "festoon" cloud, these being among the most striking examples in respect to outline, and possessing at the same time considerable value as weather prognosticators. Unfortunately the illustrations which accompanied that article were accidentally omitted, and sketches are therefore appended to the present paper for future reference.

At the present time we propose to bring before our readers some other forms of cumulus of somewhat rarer occurrence, and for the most part attendant on unsettled weather.

In the first place, then, we call attention to a form which may be denominated "pyramid cumulus." Strictly speaking, it should be "inverted" pyramid cumulus, as the point or apex is generally *downwards* toward earth, or inclined at an angle with the horizon. The mass of the cloud, which is often very irregularly shaped at the larger end, partakes for the most part largely of the character of stratus or nimbus, and the surface is often marked by sinuous lines, which radiate from the apex upwards (fig. 1).

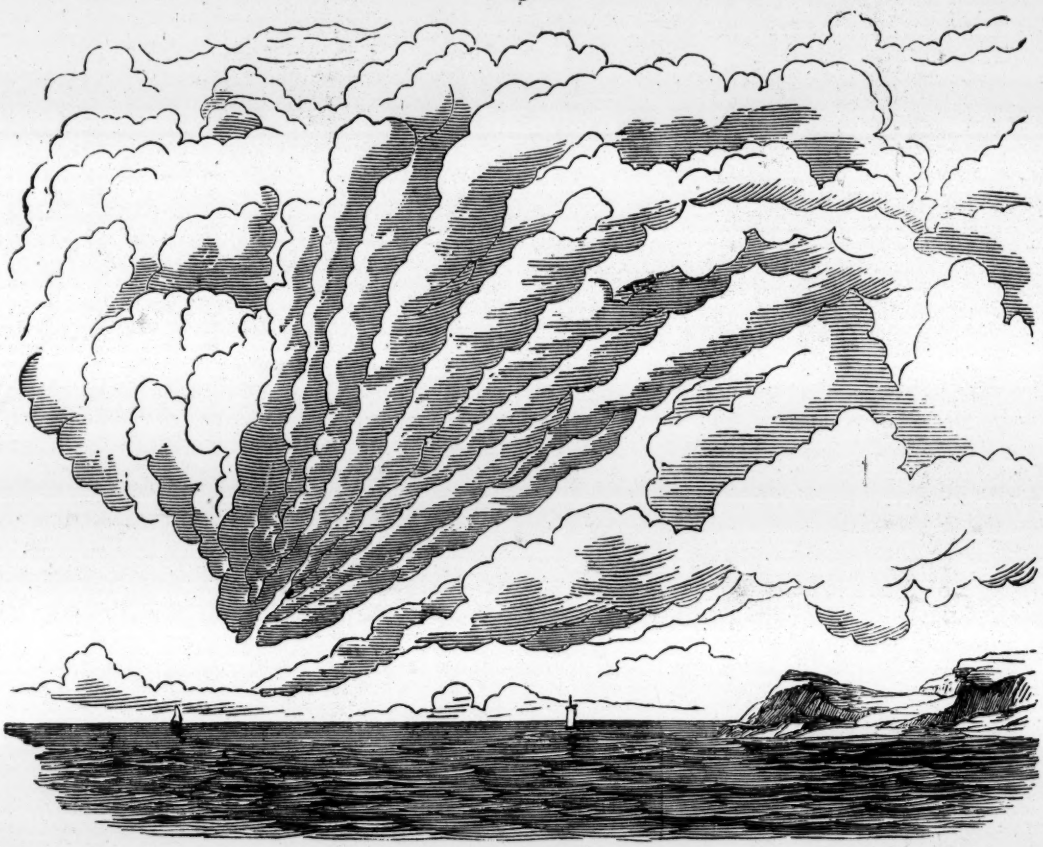
The cases in which I have observed this form of cloud have generally been during a thunderstorm, about its climax, or after the rain has begun to fall; one of the most striking instances, from which the illustration was taken, occurred during a storm which discharged itself about thirty miles from the point of observation, near Liverpool. Like the ordinary electric cumulus,* the pyramid cumulus sometimes rises to a great elevation, and is visible at a vast distance, when the lightning may be seen darting and playing about its surface; thus indi-

* *See* sketch, p. 247; and "Journal of Meteor. Soc." January 1872.

cating the violence of the storm over which it hovers, while the weather in the neighbourhood of the observer may be perfectly fine.*

This cloud derives interest from its evident connexion with that conical or funnel-shaped form of cumulus which attends the formation of a waterspout; and thus gives an indirect confirmation to the theory that this latter phenomenon is a result of electrical action. It seems quite evident that the

Fig. 1.



INVERTED PYRAMID CUMULUS.

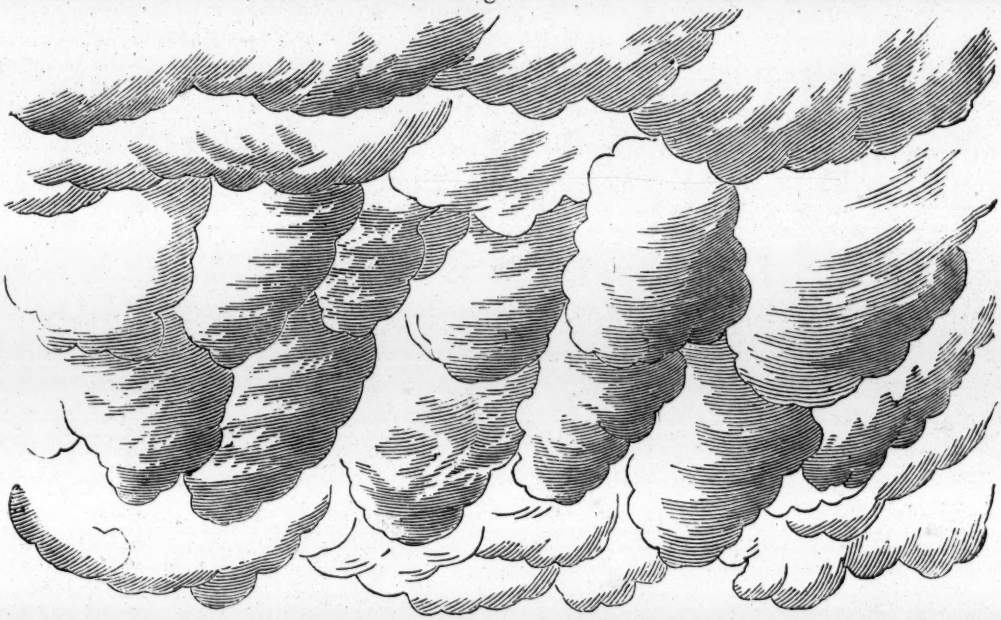
violent action, both on the water and on the atmosphere, which is exerted by a waterspout, and the tremendously rapid movement of the particles, can only result from the presence of a force acting with enormous power *at a limited distance.*

To afford a *raison d'être* for this phenomenon, and for that of the sand-pillar, from the ordinary mechanical laws regulating large masses of air, seems an obvious impossibility. But it may be objected that, if the pyramid cumulus and other cognate cloud forms are due to electric action of the air or vapour particles, how

* A case of this kind occurred about the year 1869, and was described by me in the "Intellectual Observer."

does it happen that the surface of these clouds and their general contour do not change more rapidly? To this it may be replied, that if an observer were so placed as to obtain a distinct and near view of the *component particles* of the cloud, he might (and probably would) observe a *very rapid motion* among them. For, at a distance of several miles, appearances are very deceptive as to material composition,* movement of particles, &c.; and it is also plain that identity of *visible* form at two different moments is no proof whatever of an identity of the component particles, or (perhaps I should say) of a *continuity* of composition. Take, for example, the rainbow, which exhibits such exquisite symmetry and such apparent stability

Fig. 2.



FESTOON CLOUD.

of form as altogether to preclude the idea of change or motion, while it is well known that the bow and its colouring are due to refraction of light through a constant succession of falling drops, so that not only do two persons see different rainbows, but no one can see the same rainbow two moments together.

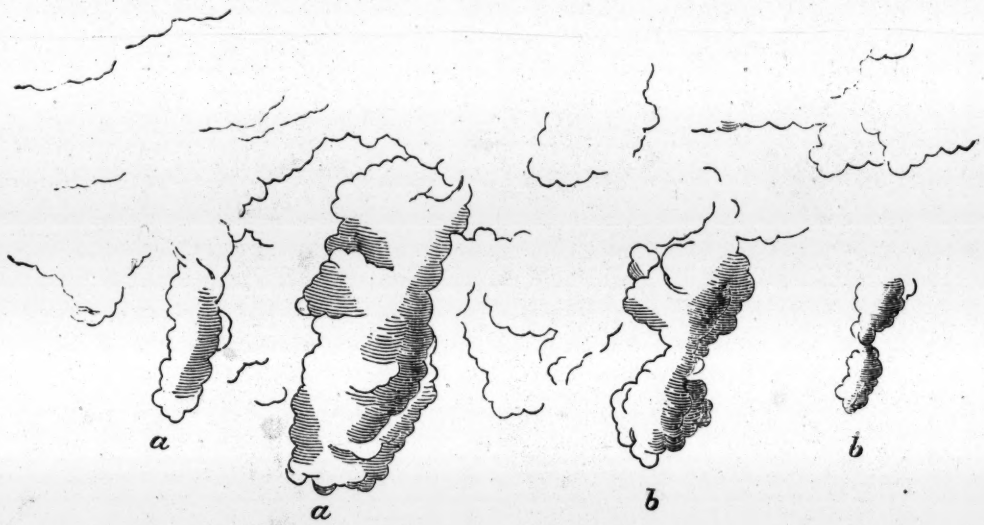
In the symmetrical marking which the pyramid cumulus assumes at times, we have a remarkable resemblance to that which is occasionally taken by the whirling sand-pillar of the desert, when the latter takes a compound form. A kind of complex

* A singular phenomenon may often be noticed in connection with the fall of rain, which has not been fully explained, viz.: the darting to and fro of small particles among the larger drops, like minute snowflakes flitting about in defiance of gravitation.

whirlwind is evidently the originating force, but whether the direction of the currents is originally upwards or downwards I must leave to other observers to determine. Perhaps we may find the true explanation of some of the more striking cloud forms in the action of two vapour currents of different densities meeting each other; but in the cases we have just considered, where a rapid motion is observed while the atmosphere all around is still, I think we may find a solution by applying the principle of correlation of forces, *i.e.* in the generation of electricity by heat or evaporation. The "festoon" cloud (fig. 2) which is commonly seen before squally weather and with a falling temperature, may, however, be due to ascending currents of vapour meeting colder currents of air descending towards the earth.

This may, perhaps, be also the origin of the next form of

Fig. 3.



DROPPING UMULUS.

a. Ordinary Forms. *b*. Accidental varieties in the shape of a dumb-bell.

cumulus which we take for examination, viz., the "dropping" cumulus (fig. 3). I am not aware that previous writers have considered this a distinct species; but it deserves to be so recognized for two reasons, viz., its value as a weather prophet (indicating wind and rain); and the curious inquiries which it suggests concerning the coherence of cloud masses, and the relation which the forces that determine this coherence bear to that of gravitation. A very ancient writer* has pointed out the scientific problems involved in this adjustment of mechanical laws, as yet far from solution.

There is a close affinity between this form and the "festoon"

* Book of Job, chap. xxxvii. v. 16.

cloud: the latter ought perhaps to be regarded as a variety of the "dropping" cumulus. The most curious characteristic of the cloud we are now considering is its manner of hanging down *endwise* towards the earth (fig. 3). It appears at times to have a hollow space within; for a kind of outward shell may be seen to form in the first instance, gradually developing from a vapoury strip or band, and rapidly assuming a semi-cylindrical shape (almost that of a bolster), and acquiring at last a full rotundity of outline. I have observed several masses formed in this way, parallel to each other, and all in different stages of development.

One day last autumn I saw several masses of this cloud (off Ramsgate), November 8. On the two following days there was a gale; the first being also very wet.

Among the accidental varieties of "dropping" cumulus, two curious forms may be noted: viz. that of a double cone and of a dumb-bell (fig. 3 *b*). The last-mentioned may be closely allied to the "anvil" cloud, but is looser in texture and generally more rapid in motion and change of form.

It has been remarked by some observers that the characteristics of the rarer clouds are very *local*, but it must be borne in mind that the same result will be observed where the *conditions* are the same. The varieties which we have just been considering have been seen by me in various parts of England, and I have taken notes for many years, chiefly in the neighbourhood of Liverpool and of Ipswich.

There seems to be a difference in the average altitude of clouds on the west and east coasts of England, those of the latter having the greater elevation. The estuaries of the Mersey and of the Dee present a very interesting field of observation; the varieties of cloud being numerous, the changes of form rapid, and the gradations of colouring and of light and shade rich and deep. To account for these phenomena we have: 1. The irregularity of the coast-line, with two considerable rivers lying close together; 2. The hills of North Wales; 3. The intermingling of currents, and the evaporation from the Gulf Stream. The part of East Kent—viz. that between Sandwich and Dover—in which I have lately made observations, presents finer effects in the rich glow of the upper sky, especially before sunrise and after sunset; a clear yet soft and rich golden light, into the depths of which the eye seems to penetrate, at times intensely beautiful. This glow was seen on several occasions in November last, 1877. The purity and depth of the ruby-tinted cirro-stratus at sunset is also very noticeable. The fine effects of cloud about Liverpool were, I believe, well known to Turner, who spent many a day making studies in the district. The excess of

vapour arising from flats and sandbanks gives a hazy and delicate effect to the light, very suggestive in a picture.

With regard to these differences of atmospheric effect caused by change of locality, it appears that such change is found rather in the grouping, elevation, and effects of light and colour, than in the forms themselves, though there are certain forms, no doubt, perfectly local, especially in mountainous districts. These generally owe their origin to geographical configuration; and their study lies as much within the province of the physical geographer as that of the meteorologist. In passing, however, I may remark that the curious disc or cap which is seen near the peak of some mountains, and which maintains its form in a breeze of wind, must have its particles in rapid motion; being caused by the continuous impact of vapour currents upon a colder stratum, which determines condensation and visibility. This is a good example of the principle stated above, that identity of visible form at two different moments is no proof of continuity of composition. The flame of a candle, which cannot, chemically, remain the same two moments together, affords another illustration of this. A cloud, like the human body, may often be likened to an eddy. Physiology teaches us that the body is sustained by a constant motion of particles drawn in from external sources and passed off again after undergoing a change of form; yet no one doubts his possessing the same body which he had, say, ten years ago. The saying, that "sameness" must not be confounded with "this-ness" and "that-ness," or *identity*, in its strictest sense, is applicable in both cases. Every cloud, as has been remarked by Espy, is either a forming or a dissolving one; but I think we may extend this, and say that some clouds are both "forming" and "dissolving" at the same time: e.g., cirro-cumulus forming from cirrus and melting into nimbus.

But we now pass on to consider a form of cumulus which is associated with very wet weather, and which I do not think has yet had a distinguishing title accorded to it. This I propose to call the "high-rain" cumulus. It is not a common species, and is mostly seen in autumn and spring. It deserves the name that I have given it, on account of the tremendously heavy showers that attend its appearance and the great elevation which it often attains. Unlike most forms of cumulus, which it is best to observe when somewhat near the horizon, so that the vertex may be seen in outline, this cloud reveals its characteristics best when almost overhead. Let the reader picture to himself a mild, muggy day in spring or autumn, everything soaking with recent showers, with heavy, lumpy-looking cumulus hanging about the horizon, and a drift of flattish, irregular cirro-cumulus

occupying *the upper stratum*.* Some of the masses of cumulus will be observed to throw off limbs or projections which swell out, and, ascending almost to the upper drift, seem to creep over the zenith much in the manner of a thunder-cloud when the electricity is gathering just over the place of observation. The cloud then forms into a kind of sheet (as seen from below) with rounded outline, and the edge broken with many small, curling indentations, somewhat like the leaves of several plants. The lower portion has a flat appearance, as if repelled, rather than attracted, by the earth. The elevation is considerable, and the tint of the cloud a dirty grey or slate-colour.

This kind of cloud frequently forms in enormous masses, as may be seen from its great altitude and the manner in which it spreads out towards the horizon; but, though much flatter in form than most varieties of cumulus, its surface is often broken or nodulous, like that of hail cumulus or electric cumulus when these are ready to discharge their contents. The peculiar form of the species we are considering is probably due in part to the saturated condition of the lower atmosphere at the time of its occurrence; and I have given to it the special designation of "rain" cumulus to indicate the excessively wet weather that it is generally associated with.

One of the most remarkable facts in connexion with atmospheric phenomena is this—that certain characteristic forms of cloud find their habitat within a certain zone, or at certain distances from the earth's surface. Taking the three main types (and omitting stratus), we find them usually in the following order, ascending from the earth: 1. Cumulus. 2. Cirro-cumulus, 3. Cirrus. The second of these appears to be an intermediate stage between cirrus, the ice-cloud in its normal state, and cumulus; or between cirrus and nimbus; and it has a very great range of altitude. Thus it might appear to be an exception to the law, that form depends partly on elevation; but it will, I think, be admitted that, as a general rule, it occupies a region lying between that of the other two species. But there are doubtless relations existing between the banks of cloud themselves which affect the question of form, as well as mere height above the earth. This especially would be the case where there was strong interchange of electrical action.

But whatever may be the *cause* of the fact we speak of, one thing in connexion with it deserves attention, viz., that any decided deviation from the law is significant of a disturbed condition of the atmosphere, and *may* be indicative of approaching storms. Let us take two cases to illustrate this: In very thundery weather, the two great types of cloud show a tendency

* See remarks below on the relative height of different species of cloud.

to interchange their places in regard to altitude; that is to say cumulus rises to an abnormal height, and cirrus descends to an unusually low position. This may generally be verified by watching the course of a thunderstorm; when the peaks of the first-mentioned cloud may often be observed, at a great distance, thrusting their pale yellow pinnacles into the azure of the heavens, long before the first muttering of the storm is heard, and while the mass of cloud is far beyond the horizon. On the other hand, when the hurly-burly is over and the rain has ceased, masses of nimbo-cirrus may at times be seen throwing up their silvery crests and curling over the earth like gigantic breakers. This last-mentioned appearance, which has a splendid effect, is, I believe, never seen except in thundery weather, or rather during electrical disturbance, and it is not easy to account for the existence of such forms by the action of wind. This remark applies also to what we may call "star" cirrus; that is, cirrus streaks radiating from a common centre in all directions (a form of cloud also connected with thunder-storms); and to that form which resembles the feathering of an arrow—parallel rays running from both sides of a common axis.

THE EXTINCT BRITISH WOLF.

By J. E. HARTING, F.L.S., F.Z.S.

(*Continued from* p. 154.)

Historical Evidence.—Scotland.

In a preceding page it was incidentally remarked that the wolf survived in Scotland to a much later date than was the case in England. The reason is pretty obvious. Long after the animal had been extirpated in England the condition of the country in North Britain remained eminently suited to its nature. Vast tracts of forest and moor, rugged and well-nigh impenetrable in parts, entire districts of unreclaimed and uncultivated land, the absence of roads, and the consequent difficulty of communication between scattered and thinly populated hamlets, long contributed to shelter the wolf not only from final extinction but from the incessant persecution which had driven it from the south.

The aspect of the country in Scotland at the date to which we refer may be imagined from a remark of John Taylor, the Water Poet, who in 1618 travelled on foot from London to Edinburgh. When visiting Braemar, he says, "I was the space of twelve days before I saw either house, cornfield, or habitation of any creature, but deer, wild horses, wolves, and such like creatures, which made me doubt that I should never have seen a house again."

It must not be supposed, however, that the wolf at any time lived unmolested in Scotland. As the herdsman's foe, it was always regarded as a beast to be pursued and killed whenever and wherever practicable, and from the earliest times the chase of the wolf was considered by kings and nobles to be one of the most exciting and enjoyable of field-sports.

We learn from Holinshed that Dorvadil, the fourth king of the Scots, "set all his pleasure on hunting and keeping of houndes and greyhoundes, ordayning that every householder should find him two houndes and one greyhounde. If a hunter chanced in following the game to lose an eye or a limme, so that he were not able to helpe himselfe after that time, he made

a statute that he should be founde of the common treasury. He that killed a wolf should have an oxe for his paines. This beast, indeed, the Scottish men even from the beginning used to pursue in al they might devise, because the same is suche an enemie to cattayle, wherein consisted the chiefest portion of all their wealth and substance." *

Of a later king, Ederus, we are told that his "chiefe delighte was altogyther in hunting and keeping of houndes and greyhoundes, to chase and pursue wild beastes, and namely the woolfe the herdsman's foe, by means whereof his advancement was muche the more acceptable amongst the nobles, who in those dayes were whollye gyven to that kynde of pleasure and pastyme."†

Ferquhard II., who died A.D. 668, is said to have proved so bad a king that Colman, Bishop of Lindisfarne, declared the vengeance of God would overtake him. "And sure his wordes proved true: for within a moneth after, as the same Ferquhard followed in chase of a wolfe, the beast being enraged by pursuite of the houndes, flew back uppon the king, and snatching at him, did wounde and byte him righte sore in one of his sides, immediately whereupon, whether through anguishe of his hurt, or by some other occasion, he fell into a most filthie disease" (phthiriasis).‡

The sport enjoyed in Scotland in former days must have been incomparable. Bellenden, the translator of Hector Boece, says, that in the forests of Caledonia there were "gret plente of haris, hartis, hindis, dayis, rais, *wolffis*, wild hors, and toadis," (foxes), and he particularly mentions "the *wolffis*" as being "rycht noysum to the tame bestiall in all partis of Scotland."

In the reign of Malcolm IV. (1153–1165) Robert de Avenel granted to the monks of Melrose the right of pasturage in his lands in Eskdale, reserving to himself the privileges of the feudal baron, to pursue the wild boar, the deer, and the stag. One of his successors questioned several of the claims to which the grantees considered themselves entitled, and it was ultimately decided in 1235, in presence of King Alexander II., that they had no right to hunt over the lands in question, and were restricted from setting traps, *excepting for wolves*.§ It seems that, in order to protect their flocks, the monks of Melrose were in the habit of setting traps for wolves as early as the reign of William the Lion (1165-1214).‖ Wolfclyde, a part of the barony of Culter, in Lanarkshire, passed by grant to the Abbey of Melrose in 1431.¶

* Holinshed's Chronicles of Scotland, 1577, p. 13.
† Holinshed, tom. cit. p. 27.
‡ Holinshed, p. 148.
§ Morton's Monastic Annals of Teviotdale, pp. 273, 274.
‖ Chalmers' Caledonia, ii. p. 132. Chart. Mel. 91.
¶ Morton, op. cit. p. 276.

In a grant of Alexander II. (1214–1249) to the monks of Melrose, in Ettrick Forest, mention is made of "Wulfhope," a name still familiar in the south of Roxburghshire. *

In 1283, there was an allowance made for one hunter of wolves at Stirling. †

In 1427, in the reign of James I. of Scotland, an Act was passed for the destruction of wolves in that kingdom. Further Acts with the like object were passed in 1457, in 1525, and in 1577. The Act of 1525, however, is merely a modernized version of the law of 1427, which is referred to in the statute of 1577 as "the auld act made tharon."

The law required "that ilk baron within his barony in gangand time of the year sall chase and seek the quhelpes of wolves and gar slay them. And the baron sall give to the man that slays the woolfe in his barony and brings the baron the head, twa shillings. And when the baron ordains to hunt and chase the woolfe, the tenants sall rise with the baron. And that the barons hunt in their baronies and chase the woolfes four times a year, and als oft as onie woolfe beis seen within the barony. And that na man seek the woolfe with schott, but allanerly in the time of hunting them." The duty of summoning the people for a wolf-hunt devolved upon the "schireffs" or "bailyis," three times a year, between St. Mark's Day (April 25th) and Lammas (August 1st), for, as the Act states, "that is the tyme of their quhelpes." The penalty for disregarding this summons was "ane wedder," "quhatever he be that rysse not." On the other hand, it was enacted that whoever slew a wolf "sall haif of ilk householder of that parochin that the woolfe is slayne within, a penny."

The Act of James the Second's time (1457), provided that "they that slayis ane woolfe sall bring the head to the schireffe, baillie, or baronne, and he sall be debtour to the slayer for the summe foresaide. And quhatsumever hee bee that slayis ane woolfe, and bringis the head to the schireffe, lord, baillie or baronne, he sall have sex pennyes." ‡

In some active instances, the exertion of these statutes might have cleared local districts, and a remarkable example of success was given by a woman—Lady Margaret Lyon, Baroness to Hugh third Lord Lovat. This lady having been brought up in the low country, at a distance from the wolves, was probably the more affected by their neighbourhood, and caused them to be so vigorously pursued in the Aird, that they were exterminated

* Chalmers' Caledonia, ii. p. 132.

† Innes' "Scotland in the Middle Ages," p. 125.

‡ "Laws of the Parliament of Scotland," folio, 1781, pp. 18, 19. *See* also Glendook's Scots Acts, 7 James I. c. 104, and 14 James II. c. 88.

out of their principal hold in that range. According to the Wardlaw MS., "she was a stout bold woman, a great huntress; she would have travelled in our hills a-foot, and perhaps outwearied good footmen. She purged Mount Caplach of the wolves." Mount Caplach is the highest range of the Aird running parallel to the Beauly Frith, behind Moniach and Lentron. Though the place of the lady's seat is now forgotten, its existence is still remembered, and said to have been at a pass where she sat when the woods were driven for the wolves, not only to see them killed, but to shoot at them with her own arrows. The period of her repression of the wolves is indicated by the succession of her husband to the lordship of Lovat, which was in 1450, and it is therefore probable that the "purging" of Mount Caplach was begun soon after that date.*

Such partial expulsions, however, had little effect upon the general "herd" of wolves, which, fostered by the great Highland forests, increased at intervals to an alarming extent. During the reign of James IV. (1488-1513), rewards continued to be paid for the slaughter of wolves in Scotland, and we learn the value of a wolf's head in those days from the accounts of the Lord High Treasurer.† For instance, under date "October 24th, 1491," we find this entry:—

> "Item, til a fallow brocht
> ye king ij wolfis in Lythgow . . . Vs."

In the time of James V. their numbers and ravages were formidable. At that period great part of Ross, Inverness, almost the whole of Cromarty, and large tracts of Perth and Argyleshire, were covered with forests of pine, birch, and oak, the remains of which continued to our time in Braemar, Invercauld, Rothiemurchus, Arisaig, the banks of Loch Ness, Glen Strath-Farar, and Glen Garrie; and it is known from history and tradition that the braes of Moray, Nairn and Glen Urcha, the glens of Lochaber, and Loch Erroch, the moors of Rannach, and the hills of Ardgour were covered in the same manner.‡ All these clouds of forests were more or less frequented by wolves. Boethius mentions their numbers and devastation in his time,§ and in various districts where they last remained, the traditions of their haunts are still familiarly remembered. Loch Sloigh

* MS. History of the Frasers, in the library of Lord Lovat (p. 44). Also the curious account of the North Highlands called the Wardlaw MS. in the possession of Mr. Thompson, Inverness (p. 67).

† Extracts from these accounts will be found in Pitcairn's "Criminal Trials in Scotland," Vol. i. p. 116.

‡ MacFarlane's Geographical Collections. MS. Bibl. Facult. Jurid. ii. 192. Quoted in Smart's "Lays of the Deer Forest."

§ Scot. Hist. fol. 7.

and Strath Earn are still celebrated for their resort, and in 1848 there were living in Lochaber old people who related from their predecessors, that, when all the country from the Lochie to Loch Erroch was covered by a continuous pine forest, the eastern tracts upon the Blackwater and the wild wilderness stretching towards Rannach were so dense and infested by the rabid droves, that they were almost impassable.*

In 1528 the Earl of Athol entertained the king, James V., with a great hunt which lasted three days. "It is said, at this tyme, in Atholl and Stratherdaill boundis, thair was slaine threttie scoir of hart and hynd, with other small beasties, sich as roe and roebuck, *woulff*, fox, and wild cattis." †

A story is told of one John Eldar, a clergyman of Caithness, who journeyed on the death of James V. to England to present to Henry VIII. a project for the union of the two kingdoms. Being asked to explain the meaning of the name "redshanks," at that time given to the Highlanders, he said, "They call us in Scotland, 'redshanks,' please it your Majesty to understand, that we of all people can tolerate, suffer, and away best with cold: for both summer and winter (except when the frost is most vehement) going always bare-legged and bare-footed, our delight and pleasure is in hunting of red deer, *wolves*, foxes, and graies [badgers] whereof we abound and have great plenty. Therefore, in so much as we use and delight so to go always, the tender, delicate gentlemen of Scotland, call us 'redshanks.'" ‡

Harrison, who wrote in Elizabeth's time, says that though the English "may safelie boast of their securitie in respect to wild animals, yet cannot the Scots do the like in everie point within their kingdome, sith they have greevous woolfes and cruell foxes, beside some other of like disposition continuallie conversant among them, to the general hindrance of their husbandmen and no small damage unto the inhabiters of those quarters." §

William Barclay, who was a native of Aberdeenshire, and spent the early part of his life at the court of Queen Mary, accompanied her Majesty on an excursion to the Highlands, and has left a curious account ‖ of a royal hunt at which he was present, and which was organized for the Queen by John, fourth Earl of Athol, in 1563. Two thousand Highlanders were employed to drive all the deer from the woods and hills of

* Stuart's "Lays of the Deer Forest," Vol. ii. pp. 231, 232.

† Robert Lindsay, "Chronicles of Scotland," ii. p. 346.

‡ Pinkerton's "History of Scotland," ii. p. 396.

§ Harrison's "Description of England," prefixed to Holinshed's "Chronicle," i. p. 378.

‖ "De Regno et regali Potestate," &c., 4to, 1600, p. 279.

Athole, Badenach, Mar, Moray, and the surrounding country. After mentioning incidentally that the Queen ordered one of the fiercest dogs to be slipped at a wolf—"*Laxatus enim reginæ jussu, atque immissus in lupum, insignis admodum ac ferox canis*,"—Barclay concludes his account of the "drive" with the statement that there were killed that very day 360 deer, 5 wolves, and some roes.

According to Holinshed, wolves were very destructive to the flocks in Scotland during the reign of James VI. in 1577. At this time they were so numerous throughout the greater part of the Highlands, that in the winter it was necessary to provide houses, or "spittals" as they were termed, to afford lodgings for travellers who might be overtaken by night where there was no place of shelter. Hence the origin of the Spittal of Glen Shae, and similar appellations in other places.

Camden, whose "Britannia" was published in 1586, asserts that wolves at that date were common in many parts of Scotland, and particularly refers to Strathnavern.

"The county," he says, "hath little cause to brag of its fertility. By reason of the sharpness of the air it is very thinly inhabited, and thereupon extremely infested with the fiercest of wolves, which, to the great damage of the county, not only furiously set upon cattle, but even upon the owners themselves, to the manifest danger of their lives. In so much that not only in this, but in many other parts of Scotland, the sheriffs and respective inhabitants are bound by Act of Parliament, in their several sheriffdoms, to go a hunting thrice every year to destroy the wolves and their whelps." *

Bishop Lesley, writing towards the close of the sixteenth century, complains much of the prevalence of wolves at that period, and of their ferocity.†

"About this time there was nothing but the petty flock of sheep, or herd of a few milk-cows, grazed round the farm-house, and folded nightly for fear of the wolf, or more cunning depredators." ‡

Towards the end of the sixteenth and beginning of the seventeenth centuries large tracts of forests in the Highlands were purposely cut down or burned, as the only means of expelling the wolves which there abounded.

> "These hills and glens and wooded wilds can tell
> How many wolves and boars and deer then fell."
>
> *Campbell's* GRAMPIANS DESOLATE, p. 102.

* Camden, "Britannia," Vol. ii. p. 1279. The editor, Dr. Gibson, Bishop of Lincoln, has a marginal note to this passage "No wolves now in Scotland" (1772). Gough's edition, 1787, Vol. ii. p. 445.

† "De Origine, Moribus et Rebus Scotorum."

‡ Irvine's "Scotch Legal Antiquities," p. 264.

"On the south side of Beann Nevis, a large pine forest, which extended from the western braes of Lochaber to the Black Water and the mosses of Rannach, was burned to expel the wolves. In the neighbourhood of Loch Sloi, a tract of woods nearly twenty miles in extent was consumed for the same purpose." *

John Taylor, the Water Poet, who made his "Pennyles Pilgrimage" into Scotland in 1618, saw wolves in Braemar. He writes: "My good Lord of Mar having put me into shape, I rode with him from his house, where I saw the ruins of an old castle, called the castle of Kindroghit. It was built by King Malcolm Canmore (for a hunting house), who reigned in Scotland when Edward the Confessor, Harold, and Norman William reigned in England. I speak of it because it was the last house that I saw in those parts; for I was the space of twelve days after before I saw either house, cornfield, or habitation of any creature, but deer, wild horses, *wolves*, and such-like creatures, which made me doubt that I should never have seen a house again." †

Years later, as we learn from Sir Robert Gordon, the wolf was still included amongst the wild animals of Sutherlandshire. He says the forests and "schases" in that county were "verie profitable for feiding of bestiall, and delectable for hunting, being full of reid deer and roes, *woulffs*, foxes, wyld catts, brocks, skuyrells, whittrets, weasels, otters, martrixes, hares, and fumarts." ‡

In 1621 the price paid in Sutherlandshire for the killing of one wolf according to statute was 6*l*. 13*s*. 4*d*.

Wolf-skins are mentioned in 1661 in a Customs Roll of Charles II., § whence it appears that two ounces of silver were paid "for ilk two daker." ‖

Twenty years later, if we are to credit the statement of Sir Robert Sibbald, whose "Scotia Illustrata" was published in 1684,

* Notes to Sobieski Stuart's "Last Deer of Beann Doran." *See* his "Poems" published in 1822 under the assumed name of James Hay Allan.

† "The Pennyles Pilgrimage, or the Moneylesse Perambulation of John Taylor, alias the King's Majesties Water Poet. How he travailed on foot from London to Edenborough in Scotland. With his description of his entertainment in all places of his journey and a true report of the unmatchable hunting in the Brea of Marre and Badenoch in Scotland." 4to, London, 1681.

‡ "Genealogical History of the Earldom of Sutherland, from its origin to the year 1630."

§ *See* Glendook's "Scots Acts, Charles II.," p. 36.

‖ The word "daker" or "dicker" is still in use in the leather trade, and means a roll of ten skins. It was anciently spelt "dyker" or "dykker," and the market toll was a penny each "dyker."

the animal had become extinct. His words are: *Lupi olim frequentes erant, quidam etiam de Caledoniis ursis loquuntur. Sed horum genus deletum et ex insulâ exterminatum est.*" *

Pennant states that the wolf became extinct in Scotland in 1680, when the last of the race was slain by Sir Ewen Cameron of Lochiel. He adds that he had travelled "into almost every corner of that country, but could not learn that there remained even the memory of these animals among the oldest people." †

Surtees gives the date of the death of the last wolf in Scotland as 1682. ‡

From more recent investigation, however, it is clear that Sir Robert Sibbald and Pennant were both mistaken, for not only were wolves slain in Scotland subsequently to 1680, but numerous traditions concerning these animals survived in the country to at least as recent a date as 1848.

Tradition.—In a Gaelic forest lay "of a remote period, the date and author of which are uncertain," the wolf is thus referred to as inhabiting the ancient pine woods of Scotland:—

"Chì mì Sgòrr-eild' air bruaich a' ghlinn'
An goir a' chuthag gu-bìnn an dòs.
'Us gòrm mheall-àild' nam mìle guibhas
Nan *lùb*, nan earba, 's nan lon."

"I see the ridge of hinds, the steep of the sloping glen,
The wood of cuckoos at its foot,
The blue height of a thousand pines,
Of *wolves*, and roes, and elks."§

Other Gaelic names for the wolf are *madadh alluidh*, commonly used; *faol chu*, and *alla mhadadh*, all of which are composed of an epithet and a word which now means dog.‖ It is also called *faol* and *mac tire*, "earth's son." ¶

In Scrope's "Days of Deer-stalking" (p. 109) is related an adventure with a wolf that happened to Macpherson of Braekaely, when he had the charge of the Forest of Benalder, and was furnished to the author by Cluny Macpherson, chief of Clan-chattan.

* "Scotia Illustrata, sive Prodromus Historiæ Naturalis," folio, 1684, pars ii. p. 9.

† "British Zoology," Vol. i. p. 88, and "Tour in Scotland," Vol. i. p. 206. *See* also Macaulay's "History of England," Vol. ii. p. 320.

‡ "History and Antiquities of the County of Durham," Vol. ii. p. 172.

§ From "The Aged Bard's Wish," given in Stuart's "Lays of the Deer Forest," ii. p. 9.

‖ Pinkerton's "Enquiry into the Early History of Scotland," Vol. ii. p. 85.

¶ Campbell's "Tales of the West Highlands," Vol. i. p. 274.

"He sallied forth one morning, as he was wont, in quest of venison, accompanied by his servant. In the course of their travel, they found a wolf den—a wolf being at that time by no means a rarity in the forest. Macpherson asked his servant whether he would prefer going into the den to destroy the cubs, or remaining outside to guard against the approach of the old ones. The servant, preferring what appeared to be an uncertain to a certain danger, said he would remain without; but here Sandy had miscalculated, for, to his great dismay, the dam came raging to the mouth of the cave, which no sooner did he see than he took to his heels incontinently, without even warning his master of the danger. Macpherson, however, being an active, resolute man, and expert at his weapons, succeeded in killing the old wolf as well as the cubs."

This Macpherson of Braekaely was commonly called Callum Beg, or Little Malcolm; and there is reason to believe that he was one of those who fought in the famous battle of the Inch of Perth in the reign of Robert III. (1390–1406.)

In the districts where wolves last abounded, says Stuart in the "Lays of the Deer Forest," many traditions of their history and haunts have descended to our time. The greatest number preserved in one circle were in the neighbourhood of Strath Earn.

At Inver-Rua, on the Spean, and consequently within the lands of Keppach, there lived a Campbell of the Slioched Chailein Mhic-Dhonnacha, or Glen Urcha race. Although thus a tenant of one of the principal branches of the Clan Donald, and removed to the distance of forty miles from his *ceann tighe*, he continued to pay his "calps" to his blood chief, the Knight of Loch Awe. This tax was a heifer, which was paid annually, and it happened one year that a short time before it fell due, the beast was killed on her pasture and half eaten by a wolf. Campbell left what remained to tempt his return, and on the following night, watching the carcase, he shot the wolf from behind a stone. Not being able, however, to afford another "calp," he flayed the dead heifer, and sent the torn hide to MacChailein Mhic-Donnacha, with a message that it was all which he had to show for his "calp;" upon which the chief observed, that he had sent sufficient parchment to write his discharge.

This is said to have happened in the time of Sir Duncan Campbell, called "*Donacha dubh a' Currach'd*," "Black Duncan of the Hood," so called from having been the last person of his rank who wore the old Highland hood in Argyleshire, and who lived in the reign of James VI. (1567–1603.)

Several traditions relative to wolves are evidences of the accuracy with which oral relations have been transmitted through many generations, which is exemplified by the familiarity and

fidelity with which they retain allusions to objects and customs disused for two hundred years.

An example of this occurs in an account of the slaughter of a remarkable wolf killed by one of the lairds of Chisholm in Gleann Chon-fhiadh, or the Wolves' Glen, a noted retreat of these animals in the sixteenth century.

The animal in question had made her den in a "càrn," or pile of loose rocks, whence she made excursions in every direction until she became the terror of the country. At length the season of her cubs increasing her ferocity, and having killed some of the neighbouring people, she attracted the enterprise of the Laird of Chisholm and his brother, then two gallant young hunters, and they resolved to attempt her destruction. For this they set off alone from Strath Glass, and having tracked her to her den, discovered by her traces that she was abroad; but detecting the little pattering feet of the cubs in the sand about the mouth of the den, the elder crept into the chasm with his drawn dirk, and began the work of vengeance on the litter. While he was thus occupied, the wolf returned, and infuriated by the expiring yelps of her cubs, rushed at the entrance, regardless of the younger Chisholm, who made a stroke at her with his spear, but such was her velocity, that he missed her as she darted past, and broke the point of his weapon. His brother, however, met the animal as she entered, and being armed with the left-handed *lámhainn-chruaidh*, or steel gauntlet, much used by the Highlanders and Irish, as the wolf rushed open-mouthed upon him, he thrust the iron fist into her jaws, and stabbed her in the breast with his dirk, while his brother, striking at her flank with the broken spear, after a desperate struggle she was drawn out dead.

The spear and the left-handed gauntlet referred to in this tradition are arms mentioned by Spencer, Leslie, and other authorities, as characteristic of the Highlanders and Irish in the days of Queen Mary.*

It is true they retained the use of such ancient weapons as late as their muster called the "Highland Host" in 1678.† But no such remains appeared at Cilliechranchie, and it is therefore probable that the story has descended from the time of Charles II.

Another story is on record of a wolf killed by a woman of Cre-lebhan, near Strui, on the north side of Strath Glass. She had gone to Strui a little before Christmas to borrow a girdle

* Cf. Spencer's "Views of Ireland;" Derrick's "Image of Ireland;" Leslie, "De Origine, Moribus et Rebus Scotorum;" and a print in the Douce Collection, Bodl. Lib. G. vi. 47.

† Wodrow MS. Bibl. Facult. Jurid. xcix. No. 29.

(a thick circular plate of iron, with an iron loop handle at one side for lifting, and used for baking bread). Having procured it, and being on her way home, she sat down upon an old càrn to rest and gossip with a neighbour, when suddenly a scraping of stones and rustling of dead leaves was heard, and the head of a wolf protruded from a crevice at her side. Instead of fleeing in alarm, however, "she dealt him such a blow on the skull with the full swing of her iron discus, that it brained him on the stone which served for his emerging head."

This tradition was probably one of the latest in the district, and seems to have belonged to a period when the wolves were near their end. Their last great outbreak in the time of Queen Mary led to more vigorous measures, which in the time of Charles II. reduced their ranks to so small a number that in some districts their extinction is believed to have followed soon after that period. Thus, in Lochaber, the last *in that part of the country* is said to have been killed by Sir Ewen Cameron in 1680, which Pennant misunderstood to have been the last of the species in Scotland.*

Some traditionary notices there are of the destruction of the last wolves seen in Sutherlandshire, consisting of four old ones and some whelps which were killed about the same time at three different places,—at Auchumore in Assynt, in Halladale, and in Glen Loth—widely distant from each other, and as late as between the years 1690 and 1700.

The death of the last wolf and her cubs on the eastern coast of Sutherlandshire, says Scrope, was attended with remarkable circumstances.

"A man named Polson, of Wester Helmsdale, accompanied by two lads, one of them his son and the other an active herdboy, tracked a wolf to a rocky mountain gully which forms the channel of the burn of Sledale in Glen Loth. Here he discovered a narrow fissure in the midst of large fragments of rock, which led apparently to a larger opening or cavern below, which the wolf might use as his den. The two lads contrived to squeeze themselves through the fissure to examine the interior, whilst Polson kept guard on the outside.

"The boys descended through the narrow passage into a small cavern, which was evidently a wolf's den, for the ground was covered with bones and horns of animals, feathers, and eggshells, and the dark space was somewhat enlivened by five or six active wolf cubs. Polson desired them to destroy these; and soon

* In the Sale Catalogue of the "London Museum" which was disposed of by auction in April 1818, there is the following entry: "Lot 832. Wolf —a noble animal in a large glass case. The last wolf killed in Scotland by Sir E. Cameron."

after he heard their feeble howling. Almost at the same time, to his great horror, he saw approaching him a full-grown wolf, evidently the dam, raging furiously at the cries of her young. As she attempted to leap down, at one bound Polson instinctively threw himself forward and succeeded in catching a firm hold of the animal's long and bushy tail, just as the forepart of her body was within the narrow entrance of the cavern. He had unluckily placed his gun against a rock when aiding the boys in their descent, and could not now reach it. Without apprising the lads below of their imminent peril, the stout hunter kept a firm grip of the wolf's tail, which he wound round his left arm, and although the maddened brute scrambled and twisted and strove with all her might to force herself down to the rescue of her cubs, Polson was just able with the exertion of all his strength to keep her from going forward. In the midst of this singular struggle, which passed in silence, his son within the cave, finding the light excluded from above, asked in Gaelic, 'Father, what is keeping the light from us?' 'If the root of the tail breaks,' replied he, 'you will soon know that.' Before long, however, the man contrived to get hold of his hunting knife, and stabbed the wolf in the most vital parts he could reach. The enraged animal now attempted to turn and face her foe, but the hole was too narrow to allow of this; and when Polson saw his danger he squeezed her forward, keeping her jammed in, whilst he repeated his stabs as rapidly as he could, until the animal being mortally wounded, was easily dragged back and finished.

"These were the last wolves killed in Sutherland, and the den was between Craig-Rhadich and Craig-Voakie, by the narrow Glen of Loth, a place replete with objects connected with traditionary legends."*

This story was related by the Duke of Sutherland's head forester in 1848 to Mr. J. F. Campbell, who has narrated it in his "Popular Tales of the West Highlands," Vol. i. p. 273.

"Every district," says Stuart in his "Lays of the Deer Forest," "has its 'last' wolf," and there were probably several which were later than that killed by Sir Ewen Cameron.† The "last" of Strath Glass was killed at Gusachan according to tradition "at no very distant period." The "last" in Glen Urchard on the east side of the valley between Loch Leiter and Sheugly, at a place called ever since *Slochd à mhadaidh, i.e.*, the wolf's den; and the last of the Findhorn and also (as there seems every reason to believe) the last of the species in Scot-

* Scrope's "Days of Deer Stalking," p. 374.

† A portrait of this devoted partisan of the House of Stuart was exhibited at the meeting of the British Association at Aberdeen in 1859.

land, at a place between Fi-Giuthas and Pall-à-chrocain, and according to popular chronology no longer ago than the year 1743. The district in which he was killed was well calculated to have given harbour to the last of a savage race. All the country round his haunt was an extent of wild and desolate moorland hills, beyond which, in the west, there was retreat to the vast wilderness of the Monaidh-laith, an immense tract of desert mountains utterly uninhabited, and unfrequented except by summer herds and herdsmen, but when the cattle had retired abundantly replenished with deer and other game, to give ample provision to the "wild dogs." The last of their race was killed by MacQueen of Pall-à-chrocain, who died in the year 1797, and was the most celebrated "carnach" of the Findhorn for an unknown period. Of gigantic stature, six feet seven inches in height, he was equally remarkable for his strength, courage, and celebrity as a deer-stalker, and had the best "long dogs" or deer greyhounds in the country. One winter's day, about the year before mentioned, he received a message from the Laird of MacIntosh that a large "black beast," supposed to be a wolf, had appeared in the glens, and the day before killed two children, who with their mother were crossing the hills from Calder, in consequence of which a "Tainchel" or "gathering" to drive the country was called to meet at a tryst above Fi-Giuthas, where MacQueen was invited to attend with his dogs. He informed himself of the place where the children had been killed, the last tracks of the wolf, and the conjectures of his haunt, and promised his assistance.

In the morning the "Tainchel" had long assembled, and MacIntosh waited with impatience, but MacQueen did not arrive. His dogs and himself were, however, auxiliaries too important to be left behind, and they continued to wait until the best of a hunter's morning was gone, when at last he appeared, and MacIntosh received him with an irritable expression of disappointment.

"*Ciod e a chabhag?*" ("What was the hurry?") said he of Pall-à-chrocain.

MacIntosh gave an indignant retort, and all present made some impatient reply.

MacQueen lifted his plaid and drew the black, bloody head of the wolf from under his arm!

"*Sin e dhùibh!*" ("There it is for you!") said he, and tossed it on the grass in the midst of the surprised circle.

MacIntosh expressed great joy and admiration, and gave him the land called Sean-achan for meat to his dogs.

Sir Thomas Dick Lauder, in his "Account of the Moray Floods of August, 1829," tells the story of the wolf killed in that district by MacQueen of Pall-à-chrocain, but lays the

scene of the exploit in the parish of Moy in the county of Inverness, which, although within the bounds of the ancient province of Moray, is far beyond the present limits of the forest of Tarnaway.

Sir Thomas gives the very words which MacQueen is said to have used in describing to the chief of MacIntosh how he killed the wolf: "As I came through the *slochk* (*i.e.* ravine) by east the hill there," said he, as if talking of some everyday occurrence, "I foregathered wi' the beast. My long dog there turned him. I buckled wi' him, and dirkit him, and syne whuttled his craig (*i.e.*, cut his throat), and brought awa' his countenance for fear he might come alive again, for they are very precarious creatures." In reward for his bravery, his chief is said to have bestowed on him a gift of the lands of Sean-a-chan "to yield meal for his good greyhound in all time coming." Sir Thomas Lauder has preserved another tradition of the extirpation of the wolf in Morayshire, when two old wolves and their cubs were killed by one man in a ravine under the Knock of Braemory, near the source of the Burn of Newton.

In the old "Statistical Account of Scotland," edited by Sir John Sinclair, and published in twenty-one volumes between the years 1791 and 1799, a few entries relating to the wolf occur, but they are neither numerous nor important. Mr. J. A. Harvie Brown, who has lately examined the entire series of volumes for another purpose, has obligingly communicated the following particulars: "The woods in Blair Athole and Strowan in Perthshire once afforded shelter for wolves (Vol. ii. p. 465), as did also the district around Cathcart in Renfrewshire (Vol. v. p. 347). In Orkney it appears they were unknown (Vol. vii. p. 546). The wilds and mountains of Glenorchay and Innishail in Argyleshire are noted as being formerly haunted by these animals, whence they issued to attack not only the flocks but their owners (Vol. viii. p. 343). Towards the west end of the parish of Birse in Aberdeenshire there is a place in the Grampians still known (1793) by the name of the Wolf-holm (Vol. ix. p. 108). Ubster, a town in Caithness (from 'Wolfster,' Danish or Icelandic) appears to have received its name either from its being of old a place infested with wolves, or from a person of the name of Wolf (Vol. x. p. 32). In Banffshire the last wolf is said to have been killed in the parish of Kirkmichael about 1644." (Vol. xii. p. 447).

Dr. Robert Brown heard a tradition in Caithness-shire that the wood on the hills of Yarrow, near Wick, was cut down about the year 1500 by the enraged dwellers in the district on account of its harbouring wolves, and that the last wolf in that neighbourhood was killed between Brabster and Freswick in a hollow called Wolfsburn.

The place where the last wolf that infested Monteith was killed is a romantic cottage south-west of the mill of Milling, in the parish and barony of Port.*

"The devastations of Oliver Cromwell in the vast oak and fir woods of Lochaber are well known, and in 1848 the old people still retained traditions of the native clearances in the same century, when the great tracts south of Loch Treig and upon the Blackwater were set on fire to exterminate the wolves."†

In the Edderachillis district, forming the western portion of what is called Lord Reay's country, a tradition existed to the effect that wolves were at one time so numerous that to avoid their ravages in disinterring bodies from their graves the inhabitants were obliged to have recourse to the island of Handa as a safer place of sepulture.‡

The Earl of Ellesmere, referring to an extract from the journal of his son, the Hon. Capt. Francis Egerton, R.N., written in India, and relating to an apparently well authenticated story of some children in Oude who were carried away and brought up by wolves,§ says: "It is odd that the same tale should extend to the Highlands. I got a story identical in all its particulars of the wolf time of Sutherland from the old forester of the Reay, in which district Gaelic tradition avers that wolves so abounded that it was usual to bury the dead in the Island of Handa to avoid desecration of the graves."

In like manner an island in Loch Maree, Ross-shire, was for the same reason selected for a similar purpose.‖

On the western shores of Argyleshire the small isle of St. Mungo, still used as a burial place, has been appropriated to this purpose from the days when the wolves were the terror of the land, the passage between it and the mainland opposing a barrier which they in vain attempted to cross.¶

In Athole it was formerly the custom to bury the dead in coffins made of five flagstones to preserve the bodies from wolves.**

When treating of the wolf in England it was observed that many names of places compounded of "wolf" indicate in all probability localities where this animal was at one time common. The same may be said of Scotland. Chalmers cites in Rox-

* Nimmo's "Stirlingshire," pp. 745, 750.

† Stuart, "Lays of the Deer Forest," ii. p. 221.

‡ Wilson's "Voyage round Scotland," Vol. i. p. 346.

§ "Ann. and Mag. Nat. Hist.," 2nd series, viii. p. 153.

‖ Macculloch's "Western Isles," quoted in Chambers' "Gazetteer of Scotland," p. 755.

¶ Constable's "Edinb. Mag.," Nov. 1817, p. 340.

** "Statistical Account of Scotland" (1792), Vol. ii. p. 465.

burghshire, "Wolf-cleugh" in Roberton parish on Borthwick Water; "Wolf-cleugh" on Rule Water; and "Wolfhope" on Catlee-burn, in Southdean parish,* to which may be added "Wolflee" or "Woole," on Wauchope-burn; and "Wolf-keilder" on the Northumbrian border. There are also "Wolf-gill land," in the parish and shire of Dumfries, and "Wolfstan," in the parish of Pencaitland, East Lothian.†

Craigmaddie, "the rock of the wolf," in the parish of Baldernock, and Stronachon, "the ridge of the dog," in the parish of Drymen, point by their name to localities in Stirlingshire which were formerly the haunts of the wolf.

Mr. Hardy states (*l.c.*) that on the farm of Godscroft a cairn, now removed, was called "Wolf-camp." It may have been a wolf's den, or perhaps an ancient "meet" of the wolf hunters who were summoned by the sheriff in the days of the early Kings James.

He adds that in 1769 there was a farm called "Burnbrae" and "Wolfland" in the parish of Neuthorn belonging to Kerr of Fowberry. The name seems to imply that it had been held in former times by the tenure of hunting the wolf, lands thus granted being called "wolf-hunt lands," as already remarked under the head of the wolf in England.

In 1756 Buffon was assured by Lord Morton, then President of the Royal Society, "a Scotsman worthy of the greatest credit and respect, and proprietor of large territories in that country," that wolves still existed in Scotland at that date.

William Smellie, the translator and editor of Buffon's "Natural History," thus comments on this statement (Vol. iv. p. 210, note, 3rd edition, 1791), "We are fully disposed to give due weight to an authority so respectable and so worthy of credit; but we are convinced that the Count has misapprehended his lordship, for it is universally known to the inhabitants of Scotland that not a single wolf has been seen in any part of that country for more than a century past."

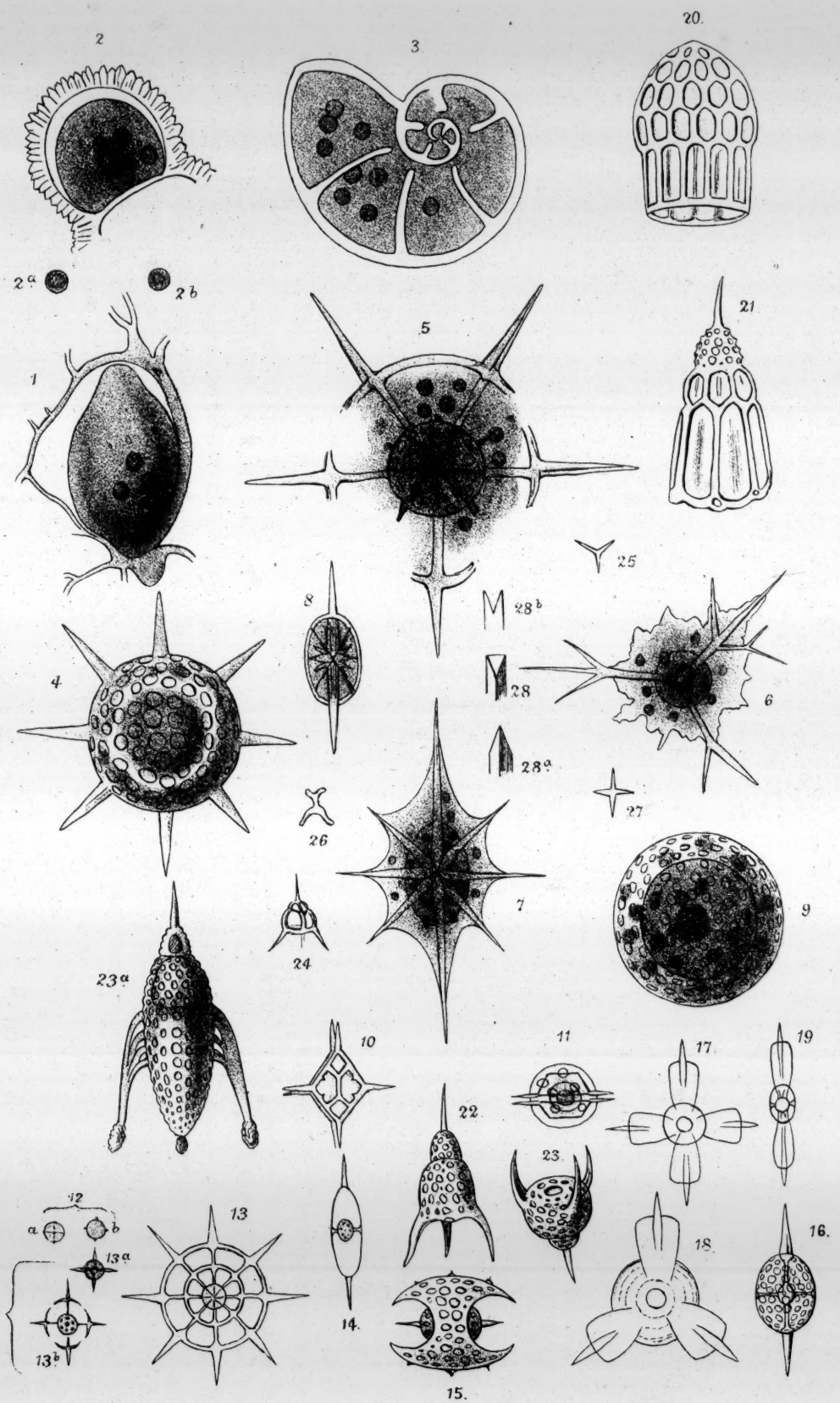
In asserting that this is "universally known to the inhabitants of Scotland" the translator and editor has erred in the other extreme, for, as has been already shown, wolves were killed in Sutherland within fifty years of the date of his remark and within thirteen years of the date mentioned by Buffon.

It now remains for us to trace the history of the wolf in Ireland.

* Chalmers' "Caledonia," Vol. ii. p. 132.

† Hardy, "Proc. Berwickshire Naturalists' Club," 1861, p. 289.

(*To be continued.*)



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TYPES OF RHIZOPOD STRUCTURE.

ON THE RADIOLARIA AS AN ORDER OF THE PROTOZOA.

By Surgeon-Major G. C. WALLICH, M.D., &c.

[PLATE VI.]

THE publication by Mr. St. George Mivart of an elaborate memoir on the Radiolaria* is undoubtedly an event in the annals of biological literature. Mr. Mivart's reputation as an earnest thinker and accomplished writer, his official position in an illustrious society devoted to botany and zoology, and his well-known antagonism to the "scientific materialism" ascribed by him to a certain school of German and also of British naturalists, are facts eminently calculated to add force and authority to observations emanating from such a quarter. But, on the other hand, they are facts in like manner calculated materially to enhance the difficulty of combating any such observations, the accuracy of which it may become necessary to impugn.

Unfortunately, on the present occasion, Mr. Mivart has not been engaged in chronicling researches carried on by himself; for, if report speaks correctly, he has never yet seen in the living or even the recent condition (from which alone trustworthy results can be secured) the singularly beautiful but fragile organisms whose biological relations he has so laudably, if not altogether successfully, undertaken to expound.

It is true that Mr. Mivart acknowledges at the outset that "his aim has been to furnish a digest and *résumé* of the most recent researches on the Radiolaria," and equally true that from first to last he has so faithfully adhered to his intention as not to have made any attempt whatever to support his inferences, professedly original and comprehensive as these often are, by a single personally attested observation. The bulk of his memoir is in reality a *résumé* of Professor Haeckel's researches on the subject, backed up here and there by supplementary facts

* "Notes touching Recent Researches on the Radiolaria." By St. George Mivart, Zoological Secretary, Linnean Society. The "Journal of the Linnean Society," Vol. xiv., No. 74, May, 1878.

gleaned from those of Cienkowski, Greef, Hertwig, and a few other continental naturalists. From data supplied almost exclusively from these sources, he has constructed a compendium of Radiolarian characters, upon which he bases his revised classification of this Order—an Order, let me observe, which, as now constituted, has only once heretofore been surpassed in the utter incongruousness of many of its elements—that is to say, in 1838, when Ehrenberg, the great pioneer of our knowledge of the lower forms of animal as well as of plant life, whilst the microscope was as yet little more than a toy, published his magnificent work "Die Infusionsthierchen."

The names of a few British and American naturalists, accompanied by a very incomplete list of their writings, are furnished at the close of Mr. Mivart's memoir. As this, with one or two unimportant exceptions, is the only direct notice accorded them, it may be fairly inferred that his estimate of their labours on the Radiolaria tallies with that recently expressed by the late Director of the "Challenger" expedition; namely, that "the Radiolaria which play so important a part in supplying the materials for new geological formations are not very familiar to British naturalists." *

It shall be my endeavour to show, incidentally, in the course of the following remarks, that British naturalists have at any rate taken their fair share in the investigation, and have only acted with a commendable caution which might with advantage have extended further, in hesitating to devote their energies to the disentanglement of an assemblage of creatures which was day by day proving itself to be more thoroughly arbitrary. Mr. Mivart would seem, however, to be unaware that until a very recent date the Radiolaria, as recognized by the majority of British writers, comprised but four families, namely, the *Actinophryna*, *Acanthometrina*, *Polycystina*, and *Thalassicollidæ*; that very detailed observations have been published by English writers on the morphological relations of each of these families; and that it is to this hour an open, though I cannot believe a doubtful, question, whether the Radiolaria, as constituted by Haeckel and now revised by Mr. Mivart, ought to be retained as a Natural Order of the Protozoa, or abolished as being one which is purely artificial, misleading, and consequently worse than useless.

Professor Haeckel is known by those who have read his works to be the unflinching advocate not only of the doctrine of *Evolution* in its most advanced sense, but of *Spontaneous Genera-*

* "Voyage of the 'Challenger.' The Atlantic." By Sir C. Wyville Thomson, Knt., LL.D., D.Sc., F.R.S.S.L. & E., F.L.S., F.G.S., &c., Vol. i., p. 231.

*tion.** Imbued with an unquestioning faith in the truth of these doctrines, he is a perfect Stoic in pressing them, through good report and bad report, to their final consequences. No writer of the present century, except Ehrenberg, has added so copiously to the literature of the lower forms of animal life; none has striven more zealously to make the most of his material. But I undertake to say none has been so blinded, both in his powers of observation and of deduction, by an overwhelming subservience to preconceived hypothesis.

As already stated, Mr. Mivart has made Haeckel's writings the almost exclusive groundwork of his memoir. Under ordinary circumstances, and in default of personally acquired experience, the ready adoption of the opinions of so distinguished a naturalist would hardly be a matter for wonder. But when there exist the strongest reasons for considering Mr. Mivart's *conclusions* as fallacious as many of the assumed facts upon which he bases them; and it can be further shown that no other writer has ever levelled a more severe indictment against Haeckel than Mr. Mivart himself within a quite recent period, an appeal to facts rather than to mere authority cannot, I should imagine, be regarded as out of place. In order, therefore, to put this statement beyond reach of question, I invite attention to the subjoined passage, one only out of several equally incisive, from Mr. Mivart's volume on "Contemporary Evolution," published just two years ago.†

"Amongst the most recent manifestations of scientific materialism may be cited Professor Haeckel's 'History of Creation.' Professor Haeckel is a very instructive writer, because his zeal for materialistic pantheism is so fiery that it hurries him sometimes into atheistic *deductions from supposed facts, which later investigations prove to have been fictions* (*e.g.* the supposed organism *Bathybius Haeckelii*‡, too pro-

* "The History of Creation." By Professor Ernst Haeckel. Translated from the German. Vol. i., pp. 234, 244, and Vol. ii., pp. 53, 278, &c.

† "Contemporary Evolution." By St. George Mivart. King & Co., London, 1876.

‡ The writer of the present article stood absolutely alone, for many years, in his endeavour to expose this monstrous fiction; and so strong was the influence of a great name, that his objections, *all* of which have since been fully borne out, were discredited. On the other hand, Professor Haeckel (the selected sponsor for this *nidus maris*), Dr. Carpenter, and Sir Wyville Thomson, forthwith received it under their patronage, and announced in the "Proceedings" of the Royal Society, almost before the first echoes of Professor Huxley's discovery had died away, that they, too, had come across *Bathybius*. living, spreading out far and wide in one vast sheet over the entire bed of ocean, and in every particular answering to the characters claimed for it. As for Professor Haeckel, he lost not a day in figuring and describing *a*

bably but a sea mare's-nest, discovered by Professor Huxley, and appropriately named by him after his German *alter ego*), sometimes into a ludicrously exaggerated estimate of the philosophical or theological consequences of elementary truths, *e.g.* those of development." (Op. cit. p. 145.)

Mr. Mivart has done me honour to mention in his list of works on the Radiolaria, published *since* the appearance of Haeckel's monograph "Die Radiolarien" in 1862, my "North Atlantic Sea-bed," published also in 1862; my paper on "The Structure and Affinities of the *Polycystina*," published in 1865; and my "Observations on the *Thalassicollidæ* and *Collosphæridæ*," published in 1869; the remark being appended to the last notice that "no mention was therein made of Haeckel's monograph." It may be allowed me, therefore, to point out that my "North Atlantic Sea-bed" was *not*, as alleged (indeed the date Mr. Mivart gives sufficiently attests this), issued *after* Haeckel's monograph; and that one reason alone deterred me from alluding to the latter in 1869—a reason Mr. Mivart compels me now to divulge—namely, the desire to avoid giving utterance to an opinion upon Professor Haeckel's method and the errors springing out of his "fiery pantheism," which, if expressed at all, could not have fallen far short of Mr. Mivart's, or been so lightly formed as to become so soon forgotten. Besides, had he taken but a little more trouble in the perusal of my paper "On the *Polycystina*," he would at once have seen that I therein, whilst referring directly to Haeckel's monograph, did my best to explain, in most guarded and courteous language, my disinclination to comment unfavourably on a method of building up a superstructure which appeared to me to be so full of danger.

I feel, therefore, that the time has arrived for demonstrating the fallacy, on the one hand, of splitting up into a number of trivial groups, and genera, and species, an assemblage of organisms so pre-eminently distinguished for an almost endless tendency to variation as the whole of the Radiolarian families; and, on the other hand, of attempting (according to a system

living specimen as seen with his own eyes. Those who desire to trace the history of the rise and fall of this prodigious fiction will find it in the following papers:—

"On the Vital Functions of the Deep Sea Protozoa," by G. C. Wallich, M.D., &c., "Monthly Micr. Journal," January, 1869.

"On the True Nature of the so-called *Bathybius*," by the same Author. "Annals and Mag. Nat. Hist.' for November 1875.

"Deep Sea Researches—The Biography of Globigerina," by the same Author. John Van Voorst, London: 1876.

"On the Coccosphere," by the same Author. "Annals," Apr. 1877.

growing far too prevalent with a certain class of naturalists) to assign a place in this already heterogeneous sub-kingdom, to every previously unknown microscopic structure that oceanic or fresh-water sources reveal to us; inasmuch as this system is already reducing the study of the lower forms of animal life to a state of chaos from which it will, probably, take years to recover. This must, I think, become manifest to anyone fairly versed in the literature of the subject, who will unprejudicedly examine Mr. Mivart's memoir, keeping constantly in recollection that the author is all the while dilating on observations made wholly by others; and that the constant appeal, for purposes of diagnosis, to the most immaterial differences; the as constant substitution of assumed for observed facts; the bolstering-up of most indefinite "definitions" by an array of qualifying and relative terms, until at last they may be made to mean anything or nothing; the creation of a ponderous, and not unfrequently not very classical nomenclature; and, lastly, the inexcusable practice of re-naming organisms, or parts of organisms, already named, described, and even figured by prior observers, are, in the main, the work of a school of Evolutionists who might with reason have been expected to utilize the motley Radiolarian assemblage, as being of all others the best-fitted for the illustration of their doctrine. Yet, incredible though it may seem, the staunchest believer in "Fixity of Species," if he happened to be unaware of the extent of variation to which the Rhizopods generally are liable, could hardly have gone further in species-manufacture. And to make the matter still more wonderful, whilst Mr. Mivart tells us, in highly eulogistic terms, that Haeckel's "Die Radiolarien" contains a description of *a multitude of new genera and species* (Memoir, p. 139); we are gravely informed by Sir Wyville Thomson that, "the Radiolaria form a class of the *somewhat negative* sub-kingdom Protozoa, which is retained for the reception of those animals of comparatively simple structure, *such as the Infusoria* (!), &c., WHOSE RELATIONS WE CANNOT VERY FULLY MAKE OUT" ("The Atlantic," Vol. i. p. 231); so that here we have the writer who, naturally enough, is regarded just now as the latest and most experienced authority on the *Radiolaria*, openly avowing that he regards them as the scientific waste-paper bag to which may be advantageously consigned all manner of creatures belonging to the lower forms of life of which we know nothing!

But it is for Mr. Mivart, the author of "Contemporary Evolution in 1876," to reconcile, if possible, the charge therein brought by him against Haeckel, with the following panegyric on Haeckel's monograph "Die Radiolarien," pronounced by Mr. Mivart, the author of "Notes touching Recent Researches on the

Radiolaria," in 1878; to wit, that "had this illustrious naturalist done no other scientific work than the publication of his magnificent and classical work 'Die Radiolarien,' that work alone would suffice to procure him everlasting fame." (Memoir, p. 139.)

Haeckel's is undoubtedly a magnificent work, and one pregnant with the results of almost unparalleled industry and study. But its utility is marred beyond redemption by the unconcealed resolve to work out, and *down* to, an all-engulfing hypothesis which renders it impossible for anyone but a professed disciple of the Haeckelian method to accept its teachings. Of course, of intentional misdirection no one would dream of charging Haeckel. Whenever he errs, it is because he is Haeckel—the slave of a dominant idea—of the "fiery pantheism" and point-blank materialism that blind him to everything but the goal he is rushing towards.

But I must not be misunderstood. In offering this commentary on the opinions entertained by Professor Haeckel, I do not even assail his hypotheses *as* hypotheses. Both of them may turn out to be true. Evolution has already been proved to be *a* method of Nature. But, without its being at all requisite to admit that Haeckel's *primordial slime* was, at some anterior epoch of the earth's history, spontaneously generated, I candidly confess I see no reason whatever for regarding it as impossible for the chemist to succeed in evolving, out of the inorganic elements and natural forces at his command, a compound capable of evincing vital activity. What I do assail is, the tendency of the Haeckelian method to gallop on altogether in advance of demonstrated fact, beyond which the scientific biologist is not warranted in advancing by a single hair's breadth. All he may legitimately do is to use his hypotheses as such only. The moment he does more, and permits himself to talk seriously of an admitted "assumption," on which he has already based a system, as having "been proved to be a necessary hypothesis"—in other words, claiming the right to say *necesse, ergo sit*, he merges at once into the mere speculator, the foundations of whose edifice are of sand.*

It is quite inexcusable enough for systematists to persist in applying a definite term to a portion of structure which belies the very definition it conveys, and to make what is little better than an unsupported assumption the basis of such an erroneous and yet positively stated dictum as the following:—"The *yellow cells* † are undoubtedly *cells* multiplying by spontaneous division of their cell contents, each division surrounding itself by its own

* See Haeckel's "History of Creation," Vol. ii. p. 278.

† See on p. 274.

cell wall before the dissolution of the mother-cell." (Memoir page 145.) But such a dictum becomes sheer nonsense after its having been gravely laid down on the authority of Cienkowski that these *yellow cells* (described in the page preceding as being "very characteristic of the whole of the *Radiolaria*, except some *Acanthometra* forms"), "were found vigorously multiplying in *dead Radiolaria*;" and that Cienkowski "suspected that they may be *parasitic organisms*;" whilst Hertwig, it is said, opposed Cienkowski's view on account of the constancy of the presence of the *yellow cells* in the *Radiolaria*, on which Mr. Mivart, with singular naïveté, remarks:—"But undoubted parasites are present with remarkable constancy in many higher animals, while several difficulties disappear if we regard them as parasites. It would (he says) first account for no other satisfactory explanation of their origin having been arrived at; secondly, for their greatly varying number; thirdly, for their survival and increase amidst the decomposition of the individuals in (*sic*) which they live; and lastly, it would explain the anomaly of *unicellular animals* containing true cells." (Memoir p. 145.)

At p. 144 it is stated that "certain peculiar structures, namely, the *yellow cells*, are very characteristic of the Radiolaria, being found in *all*, except some *Acanthometra* forms. They are nucleated, and their protoplasmic contents *are enclosed in a distinct membrane*." Whilst, in reference to the question as to whether unicellular animals can contain true cells within themselves, as part and parcel of their organization, Mr. Mivart continues as follows: "Without venturing to express an opinion on this controversy, I would place on record that Hertwig has come to the conclusion that a multinucleolate cell is potentially multinucleate, as a multinucleate cell is potentially multicellular; and *thus* we get a transition from unicellular to multicellular organisms. . . The multicellular nature of the Radiolarians now depends entirely on the normal nature of the yellow cells, and on the correctness of the observations of the centripetal cell-groups of *Physematium*. As has been said, *neither of these phenomena can be reposed on as being certainly of the nature of true cells, forming part of the normal organization of the Radiolarians* in which they have been found; but even if they are so, and if we are compelled, therefore, to regard Radiolarians as multicellular, their multicellularity is of a radically different kind from that of any of the METAZOA; and none of their parts, whether true cells or not, have any valid claim to the denomination of a *tissue*. (Memoir, pp. 182, 183.)

We shall see hereafter that their true cell nature is very distinctly maintained, nevertheless; or if not so, that the absence

of a *nuclear vesicle*, which is adopted by Mr. Mivart as the most prominent and important feature in six-sevenths of the whole of the *Radiolaria*, is contra-indicated in every one of these six-sevenths (with exception of the *Polycystina*, one group which, as I have never ceased to contend, ought not to be associated with the other groups, but with the Foraminifera in the first Rhizopod order), in consequence of this very deficiency as regards a *definite nucleus*—that is, a truly *encapsuled* nuclear mass.*

But speculative philosophy and *naïveté* go yet a stage further; for Mr. Mivart informs us that his *deus ex machinâ*, "Haeckel, *speculates* as to the possibly HEPATIC nature of the yellow cells, considering it to be not unlikely that they may be AN INCIPIENT FORM OF LIVER. But in the first place these cells may, as has been said, be PARASITIC; and secondly, a liver is, as it were, a comparatively late result of tissue formation, and could HARDLY exist in the admittedly tissueless Protozoa!" (Memoir, p. 158.)

In 1863 I pointed out, for the first time, that the so-called yellow *cells*, as also more or less perfectly colourless, but in other respects perfect homologues of them, are present throughout *all* the Rhizopodal families, both oceanic and fresh water; this statement being based on long personal experience. At the time indicated, they had been observed by others only in three *pelagic* families, namely, the *Polycystina*, the *Thalassicollina*, and the *Acanthometrina*; their brilliant yellow tint in the pelagic families being regarded as their distinguishing character, until it was shown by me that absolutely identical bodies (see Pl. VI., figs. 1–3, 5, 6), in all save colour, are common, as before stated, to the entire Class. Their office had, moreover, until then been either altogether unrecognized, or, so far as I am aware, referred to only more or less incidentally as in some manner connected with reproduction.

It was in the course of a laborious day-by-day series of observations on the fresh-water and littoral Rhizopods, extending over the greater part of the years 1863 and 1864, that I was enabled to compare and trace clearly and consecutively the mode of origin of these remarkable bodies, and to prove beyond all reasonable doubt that they constitute a true reproductive organ, formed either *directly* (as may be witnessed in the Foraminifera of our own shores) by the aggregation into minute spheroidal masses of granular, probably germinal, particles, which, up to the period of this change taking place, are more or less uniformly distributed through the sarcode mass generally: or, *indirectly*, by the subdivision of the contents of the granular nuclear mass itself, without, however, acquiring in any instance

* See my classification at the close of this article.

(as the *nucleus* of the higher order of Rhizopods invariably does) a membranous covering. For these combined reasons which had obviously made the term "*yellow cells*" a dangerous misnomer, I designated them *sarcoblasts*. Whether, in the fresh water, the littoral, or oceanic Rhizopods, *the sarcoblasts invariably constitute*, when liberated from the parent organism (as they are always eventually destined to be), either at once the infant shell-less organism, as in the naked Rhizopods; or the *nidus*, and at the same time the infant mass of sarcode, within which the rudiment of the shell or other mineral framework of the organism is secreted. Thus, in the *Amœbans*, the sarcoblasts speedily assume, one by one, the whole of the typical characters of the parent form. In the *Foraminifera* and *Polycystina*, they are the *nidus*, within which, in the former family, the primordial calcareous chamber of the shell is secreted, and in the latter family, the earliest siliceous rudiment of the siliceous framework or perforated shell; whilst in the *Acanthometrina* and other families constituting the second Order, in which an *encapsuled nucleus* first presents itself, they form the *nidus*, not only within which the axial portion of the siliceous framework is secreted, but also within the peripheral portion of which the earliest discernible vestige of the membranous capsule is secreted, in the condition of a most subtle membrane. Indeed, so fully is this fact borne out by observation that it may, I venture to believe, be laid down that until the sarcoblast has already become freed from the parent structure, no trace whatever of the nuclear capsule exists.

For the above stated reasons, (which I submit, demand something more for their refutation than speculative disquisitions about the unicellular or multicellular potentialities of the Radiolaria, or about their incipient *Liver*-systems and *parasites*), I have considered it absolutely necessary to substitute the term *sarcoblasts* for that of *yellow-cells*, as being not only suggestive of the derivation and function of these bodies, but as avoiding a condition as to colour, which is altogether untrue in the case of the fresh-water Rhizopods.

But after what has been stated concerning the opinions of Professors Haeckel and Hertwig, Sir Wyville Thomson, and Mr. Mivart himself on the nature and origin of the *yellow-cells*, it can surprise no one that the term *sarcoblasts* should not have been very readily adopted by any of these writers. Mr. Mivart tells us at the outset of his observations, that "each individual Radiolarian consists of two portions of coloured or colourless sarcode; one portion nucleated and central; the other portion peripheral and almost always containing certain *yellow-cells*; *these portions being separated by a porous membrane*, called the *capsule*" (Memoir, p. 137), it being added in a footnote

that, according to the opinion recently offered by Sir Wyville Thomson, it is probable that the *capsule* does not present itself in some of the Radiolarians.

Having for twenty years maintained that the *Polycystina* do not possess a true "definite nucleus"; or, as described by me, no true encapsuled nuclear mass; and that they must on this account rank with the Foraminifera, the sarcode bodies of which are similarly constituted, and cannot, therefore, even under the most strained interpretation of their characters, be grouped naturally with the so-called "Radiolaria;" it is requisite that I should call attention to the opinion quite recently expressed by Sir Wyville Thomson regarding these organisms, after having had nearly a year and a half to maturely weigh all the evidence he was so fortunate as to secure during his explorations on board the "Challenger."

Sir Wyville's work, "The Atlantic," was published at the close of 1877, that is to say, nearly a year and a half after the return of the expedition. At page 231 of the first volume, the subjoined far from encouraging description is given of the Radiolaria; namely that "they form a *somewhat negative class of the sub-kingdom* Protozoa, which is retained for the reception of those animals of comparatively simple structure, *such as the Infusoria*, &c., WHOSE RELATIONS WE CANNOT YET FULLY MAKE OUT."

Again, at p. 232 of the same volume, the Radiolaria are described by him as "consisting essentially of rounded or oval granular masses of brownish or yellowish colour, interspersed with very characteristic round oil-globules, bright yellow, and very refractive, the whole cemented together by soft, transparent sarcode, including fine granules. Near the centre of the body there is usually a very evident rounded mass of *bioplasm*, which colours deeply with carmine; and the same dye brings out smaller BIOPLASTS scattered irregularly through the general substance."

Thus we find on a comparison of these descriptions:—First, that the "granular masses" mentioned are not identical with the bodies afterwards referred to as "smaller *bioplasts*"; secondly, that "the characteristic round oil-globules" are not identical with the granular masses just referred to; and, thirdly,that the "*smaller bioplasts* scattered irregularly through the general substance," must necessarily be yellow cells or, in other words, SARCOBLASTS.

This portion of the Rhizopodal structure is, in reality, a small spheroidal mass of sarcode, generally varying in size in the different families within certain limits, and wholly devoid of any true membranous or other special covering. When first observable, it consists of an almost colourless, hyaline, viscid, and basal fluid, of the consistence and appearance of the white of

egg, within which are distributed peripherally, but without any approach to regularity, a number of granules of more consolidated, as well as more or less faintly coloured sarcodic substance (fig. 2 *a*); and invariably (I believe) in the case of the sarcode of the oceanic Rhizopods, of a tolerably brilliant yellow tint. But by carefully focussing down to an equatorial plane, a central portion, altogether devoid of granules, and occupied solely by the pure basal sarcode, may quite readily be detected (fig. 2 *b*). It is to this clear portion that the somewhat misleading name of *nucleus* has been applied; perhaps owing to the idea that its apparent higher degree of refractive power, as compared with the surrounding mass of the animal's sarcode, may be due to its being a specialized product; or, in other words, not identical with the basal ungranular sarcode of the rest of the sarcoblast itself, and of the general body substance in which granular particles are almost always, more or less densely, suspended. Through some subtle reproductive operation, of which we as yet have learned nothing from actual observation, in consequence of the extremely minute nature of the particles concerned, the yellow granules become, after a time, collected together into the spheroidal masses now become sarcoblasts; and, possibly, two sexual elements are, at this stage of the organism's history, brought into contact. It will, I hope, be clearly understood, however, that I throw out this view simply as a surmise, resting on no more stable basis than a fact observed by myself, and for the accuracy of which I am ready to vouch with perfect confidence, that the so-called nucleus of the sarcoblast becomes eventually, on the escape of that organ from the parent structure, the active centre of shell or skeleton-development. But on this subject I shall have more to say hereafter.

We are hence almost irresistibly led to the conclusion that the truly *constructive* "germinal" substance of the Rhizopod is the hyaline, homogeneous, viscid, and ungranular portion of the sarcode; whereas the granules and every other portion forming an integral part of the structure, are developments from this, and constitute true "formed material"; this view holding good both as regards the entire mass of the body in the youngest stage of the naked *Amœbans*; in the earliest formed chamber of the *Foraminifera*; and also in the earliest stages of the *Polycystina*, *Plagiacanthidæ*, *Acanthometrina*, and *Dictyochidæ*, in which the siliceous structure constitutes an internal framework or skeleton, rather than an external shell. Whilst throughout the entire series of the Rhizopodal families, wherever there exists a mineral fabric of any kind, whether shell, framework, groups of spicules, or shell composed partly of mineral matter elaborated by a vital operation of the creature, and partly of altogether extraneously derived mineral particles,

all are more or less invested exteriorly by a delicate film of sarcode, which (as long ago pointed out by Professor Williamson, as being characteristic of the *Foraminifera*) is an effective agent in increasing *exogenously* the thickness and extension of the mineral portion, of whatever nature the latter may be.

In short, as tersely put by Dr. Carpenter, whilst describing the perfectly developed cell-formation observable in the higher animals, the nucleus may be regarded as *the seat of the cell's formative activity.** So in the sarcoblast (which is *not* a cell, and is therefore in no respect more entitled to the appellation of the term "cell" than a globule of oil, albumen, or other viscid fluid, notwithstanding that it comprises one portion more consolidated or specialized than the other), the central region is "the seat of *its* formative activity"; this very instructive fact having been, as already stated, completely verified, by what has been observed by me to take place, both in the marine and freshwater Rhizopods.

But the great importance of arriving at a definite and correct conception respecting this part of the Rhizopod structure will presently be made apparent on other and even stronger grounds. Meanwhile it must be mentioned that Mr. Mivart attempts, amongst other things, to determine "whether or not the resemblance, insisted on by some writers, as existing between the *Radiolaria* and the *Heliozoa* is such as to render it desirable to unite them as two subdivisions of one larger group, itself distinct from all the other groups of Protozoa." Hertwig, he says, "in his latest paper on the histology of the Radiolaria, leaves the question undecided whether the *Heliozoa*, *Radiolaria*, and *Thalamophora*, should constitute three distinct and co-equal groups; or one great group should be made to comprise the *Heliozoa* and *Radiolaria* only, the other being set aside for the marine and freshwater *Thalamophora*; Hertwig having in a prior paper denied the relationship of the *Heliozoa* to the *Radiolaria*," on the ground of the supposed cell-structure both of the external and internal alveoli, and "also of the *Wasserhellen Bläschen*,† as well as on other accounts." Mr. Mivart, however, goes on to say that as Hertwig is now satisfied that "alveoli are but *vacuoles*, without membranous walls, and that the *Wasser-hellen Bläschen* are but *nuclei*, his reasoning falls to the ground;"—a conclusion in which I heartily coincide.

The similarity of the *Heliozoa* and *Radiolaria* in external form and chemical composition, Mr. Mivart regards as undeni-

* Carpenter on "The Microscope," p. 734. Fifth ed., 1875.

† These are said to be "rounded homogeneous particles of denser sarcode, devoid of investing membrane; and to be the 'simple nuclei'" of authors. (Memoir, p. 141.)

able; but, on the other hand, very many Radiolaria, according to Hertwig, depart widely from the spherical form; and similarity of chemical composition * cannot, therefore, be considered a distinction of great weight, seeing that such similarity is shared with many other lowly organisms of quite different affinities. (Memoir, p. 181.)

With reference to the reproduction of the *Radiolaria*, Mr. Mivart informs us that it "*undoubtedly* takes place by means of numerous ZOOSPORES, each with a *nucleus* and FLAGELLUM, but with no vacuoles; whereas in the *Heliozoa* the much fewer separated reproductive parts have each two *flagella* AS IN MOST RHIZOPODS (!), several contractile *vacuoles*, and a *nucleus*, with *vacuoles* and *nucleoli*." These differences he considers as so great that he warns us (as indeed he well may, considering that he does not shrink from laying down as regards the reproduction of the *Radiolaria* "by *spores*," that "it is *certain* that such a process occurs in *Acanthometra, Thalassicolla, Sphœrozoum, Collosphœra and Collozoum*, and most probably in ALL the Radiolaria." (Memoir, p. 167.) And again, that "there are also great differences in this respect between different *Heliozoa*, *while the reproductive processes of so few Radiolarians have as yet been examined, that it would be rash to feel confident that no important divergences will hereafter be found to exist among them in this respect!*" (Memoir, pp. 180, 181.)

I candidly confess to feeling confounded, and at an utter loss even up to this point to reconcile these seemingly diametrically opposed opinions; or to comprehend what "further important divergences" should be needed than the all important one to which Mr. Mivart himself draws attention, and constitutes No. 4 in his list of distinctions categorically given (at p. 180) between the *Radiolaria* and *Heliozoa*; namely that "*yellow cells* are present in almost all Rhizopods, but absent in *Heliozoa*;" that is to say if he, or rather those writers on whose opinions he is placing his trust, choose to insist that because the sarcoblasts of the freshwater *Actinophryans*, in common with the rest of

* I have been altogether unsuccessful in tracing to whose chemical analysis of these microscopically minute structures we are indebted for this apparently decided judgment upon their elementary constitution. Under all the circumstances I would humbly suggest that it will be a long time, indeed, before any *differences* in the chemical composition of such subtle organisms can be determined by any re-agents at the chemist's command. In the very nature of things, identity, or apparent identity, must be far more easily indicated. But be this as it may, when it is borne in mind that it is unreservedly asserted that "The *Radiolarians* live on Algæ, Diatoms, and Infusoria, and other small animal and vegetable organisms found near the surface of the sea," it must, indeed, be a difficult task to ascertain where chemical analysis is dealing with the mere animal's body, or with its victuals.

the freshwater Rhizopods, are not yellow, the one variety is not the true homologue of the other; or unless he really adopts the almost ludicrous view enunciated by Cienkowski that the "*yellow cells*" of the *Radiolaria* are "*parasites*."

But the climax has yet to come. It has been already stated that, according to Mr. Mivart's *résumé*, "the central nucleated portion of the Radiolarian sarcode, and the peripheral portion which almost always contains *yellow cells*, are separated by a *porous* membrane called the 'capsule';" and that he quotes Sir Wyville Thomson's opinion that this "*capsule*" is frequently altogether absent. Here are Sir Wyville's own words: "In many Radiolarians, and especially in some very peculiar compound forms, a spherical internal chamber called the "central capsule," whose functions we do not fully understand, is very prominent. *This capsule is however absent, or at all events exists in a very modified form* IN THE MORE TYPICAL *groups*." Had Sir Wyville Thomson particularized but one step further, he would probably have announced that, as always consistently urged by me, one most important family of the Radiolaria, as constituted by Haeckel, and adopted by Sir Wyville Thomson and others on Haeckel's authority which in turn probably rested on Johannes Müller's, is pre-eminently *the* typical one which possesses no encapsuled nucleus, or (to use the new designation) no "central capsule"; and that, if the classification of the Rhizopods is to be based as far as practicable on the physiological advances observable in the sarcode bodies of the animal, the absence of this "capsule" at once ratifies my conclusion that the place of the *Polycystina* is essentially alongside of the *Foraminifera* in the lowest Rhizopodal Order, and *not* with the so-termed *Radiolarians* in the second or higher Order.

But how does Mr. Mivart meet this significant, though, as already pointed out, by no means novel discovery of Sir Wyville Thomson's—a discovery, be it observed, upon the correctness or incorrectness of which the question of the propriety of associating the *Polycystina* with the families of the second Order of the Rhizopods was fifteen years ago shown by me to depend?* Is it by adducing good evidence of its error? Or by admitting that, if not an error, the entire fabric of the *Radiolaria*, as a whole, is unsound? Or by inserting in his *résumé* of all previous researches on the subject, that at least one writer on the oceanic and freshwater *Protozoa*, not only published the bare fact mentioned by Sir Wyville Thomson, but produced ample evidence in support of it? No! But by appending in a footnote the following singular commentary:—"If Sir Wyville

* In the very paper on the *Polycystina* which Mr. Mivart notices in his list.

Thomson's opinion is not a *clerical* error, some very interesting new forms may be expected to be made known by the publication of the "Challenger" Zoology; but whatever novelties may be forthcoming, forms without a central capsule should be excluded from the *Radiolaria*."

However, with two such admissions before us:—the one that the "porous membrane called the capsule," held by the supporters of the Radiolarian classification to form its most important characteristic, "is *absent*, or exists only in a very modified form in the more *typical* groups;" the other, "that forms without a central capsule should be excluded from the *Radiolaria*;" there is evidently good ground for hoping that "this somewhat negative class of the Protozoa which," according to Sir Wyville Thomson, "has been retained for the reception of animals of comparatively simple structure, such as the *Infusoria*, whose relations we cannot fully make out," may—to use an expression now rendered classical—be abolished "*bag and baggage*."

It is desirable to mention here that in such *Polycystina* as that figured at page 140 and again at page 150 of Mr. Mivart's memoir, under the name of *Eucecryphalus Schultzii*, and also in the genera *Pterocanium*, *Lithocampe* and others, the division of the sarcode-body into two or more lobes which are to be seen, in the recent condition of these organisms, projecting towards the mouth of the shell from its innermost and uppermost portion, is not due to the presence of a membranous support, as has often been erroneously imagined; but merely to the sarcode being prevented from forming itself into a single bulging mass (such as we see within the aperture of the tests of *Arcella* or *Difflugia* when the animal is at rest), by festoon-like processes sent off from the interior of the primary chamber; these processes having the effect of constricting the sarcode where it impinges on them, without, however, interfering with its free coalescence posteriorly. At the same time, if we may judge from what is observable in *Arcella*, it is probable that when the animals of the *Polycystine* genera above named are in their natural element, the lobes may disappear through the flow forwards of the dense and sometimes more deeply tinted portion, which, in captured specimens, is usually found occupying the innermost part of the shell, both in the *Polycystina* and in some species of *Foraminifera*. Accordingly, the occurrence of the lobes referred to, though furnishing a good distinctive character derived from the peculiarity in the configuration of the siliceous framework, does not indicate either structural or physiological advance. For this reason I have thought it necessary to call attention to it.

[*To be continued.*]

HOW WERE THE EOCENES OF ENGLAND DEPOSITED?

By J. STARKIE GARDNER, F.G.S.

IN this inquiry it will be necessary to pre-suppose that my readers are more or less familiar with that great series of Tertiary rocks in England known as the Eocene. I will, however, briefly recapitulate the subdivisions into which geologists have grouped it, and their several characteristics; and will then see how far the facts that have been gathered may be connected into a perfect chain of events, and a consecutive chapter in the earth's history constructed from them.

The Eocene, or "New Dawn" age, is, in England, the newest formation present in great thickness, and also spread over a wide area. We possess more recent deposits, such as the "crags" of Suffolk and Norfolk, and Pleistocene clays and gravels scattered here and there; but these are not parts of any of the great systems of strata which form, to a considerable extent, the solid crust of our earth.

Reference to geological maps of England shows that the Eocenes are at the present day almost confined to two great areas, severed from each other by Chalk ridges. One of these is known as the "London Basin," the other as the "Hampshire Basin." There are other "basins" in France, Belgium, and North Germany; and in Italy, Austria, and Switzerland there are many local Eocene deposits containing faunas, and more especially floras, which have not yet been carefully separated from the overlying and, in those countries, better developed Miocene.

It must not be supposed, however, that, in Western Europe at least, the Eocenes were originally deposited in isolated basins; but being, as they are, the uppermost deposits and loosely compacted, they have, speaking generally, only resisted denudation in the areas which have remained depressed, and have been swept off those districts which have since been elevated into ridges. There is abundant evidence to show that all these "basins" are but the remains of more or less continuous sheets of strata which formerly covered the intervening areas.

Eocène Inférieure.—The lowest members of the Eocene, known as the *Eocène inférieure* of France and Belgium, are altogether

wanting in England, and a gap of incalculable age, only slightly lessened by these on the Continent, exists between our lowest Eocene and the Chalk upon which it rests.

Thanet Sands.—These, the lowest Eocene deposits present in England, rest unconformably upon the Chalk. They are confined in England almost to the eastern area of the London Basin, but spread over Picardy and Flanders as the *Sables de Bracheux*, and in Belgium are known as the *Landénien inférieur*. They indicate a very temperate climate.

Woolwich and Reading Beds.—In the Hampshire Basin these are entirely unfossiliferous mottled clays derived from granite, and of fluviatile origin. Eastward, near London, lignites and sands with brackish-water shells come in. Further east the fluviatile mottled clay disappears, and the horizon is occupied by marine sand with estuarine shells. A flora was found near Reading, valueless for climatal inferences; but from the presence of palm wood, sharks' teeth and turtles elsewhere, it appears that the climate was perhaps even sub-tropical, but still far less hot than it became in the time of the London Clay.

Oldhaven Beds.—These consist of great beds of rolled shingle and fine sand, containing sometimes estuarine and sometimes marine shells; they are confined to the eastern area of the London Basin.

London Clay.—This is a compact mass of clay of great thickness, and spread over both the English "basins" and Belgium. It is divided into zones deposited in shallow water, which occur at the base; and deposits from deeper water, but not estimated at more than 100 fathoms, which form the upper part of the series. Only the beds characterized by shallow water mollusca are found in the western area. In addition to the marine fauna, a terrestrial fauna and a flora of great extent are known, and it is remarkable that these latter present a far more tropical assemblage than do the marine products. The beds at Sheppey appear to have formed a great river delta.

The Lower Bagshot Beds.—In the west these are pipe-clays and sands, derived, as were the older mottled clays, from granite, and deposited in fresh-water. Towards and east of London they are entirely composed of shingle and sand worn from chalk flint and left by the sea. The rich flora contained in this series at Studland and Alum Bay forms a similar group to that, the fruits of which are found at Sheppey.

The Bournemouth Series.—From Studland to a mile east of Bournemouth, these are purely fresh-water delta deposits. They are then overlain by marine or brackish delta mud, by shingle and sand, and by a sequence of marine strata at Hengistbury Head. This series is particularly interesting, as in it we can trace step by step one of the numerous subsidences which took

place in Eocene times. The flora shows a gradual change from upland-character to valley and to swamp—then lagoon conditions are traced, shingle and sea sand, and finally open sea deposits. A noteworthy fact is that in the marine beds we have fruits which, although comparatively few in number, are important, because similar to or identical with those from Sheppey.

Bracklesham Series.—A mass of marine sands distinguished lithologically from the London-Clay series, by the presence of green grains. The marine fauna is totally distinct and far more tropical than that of the London Clay, whence it is inferred that these beds were deposited in a sea shut off from the more northern London-Clay sea. They occupy most of the Hampshire Basin, and are traced in the London Basin as far as Chertsey, but their principal extension is in the Paris area. They appear in England to have been deposited in the quiet estuary of a great river, and are partly contemporaneous with the Bournemouth series.

Barton Beds.—These are marine deposits occupying a limited area. Although separated by a greater lapse of time, their mollusca far more closely resemble those of the London Clay than do those of the Brackleshams ; many species of the latter which had disappeared now reappearing. The fauna has in fact a more northern facies than the Bracklesham fauna, only twenty per cent., and these the more temperate mollusca, remaining.

The Upper Bagshot Beds.—These are mostly white siliceous sands. Mr. Godwin-Austen has shown that the sudden succession of sand to clay involves no physical change. The fauna, where present, is of the Barton type.

The Upper Eocene.—The remainder of the Eocene strata in England are of fluvio-marine origin, and 560 feet thick. They are alternations of marine, brackish, fresh-water and occasionally subaerial deposits. They are only found in the Isle of Wight, and on the contiguous main-land, and the newest beds have the most circumscribed area. The fauna and flora are tropical, but show a gradually decreasing temperature. The highest bed, which is marine, is limited to a single hill, and contains a strangely dwarfed and impoverished, but purely Eocene marine molluscan fauna. It is thought, nevertheless, by many to be contemporary with European Miocenes. The divisions, purely arbitrary, are :—*Lower, Middle, and Upper Headon, Osborne and St. Helens, Bembridge Limestone, Bembridge Marl, Hempstead* fresh water and marine. The marine beds are relatively most developed in the east, the fresh-water in the west of the island.

Having thus glanced at the salient features of the strata composing the Eocene in England, I will now endeavour to

interpret the changes in character of the deposits and their contents. I wish, however, to impress upon my readers that while the foregoing section of the subject is well ascertained fact, a portion of the remainder is, for the present, to be considered as mere theory and individual opinion. It has not previously been laid before men of science, and therefore, although it seems to me perfectly consistent, objections may be urged against my conclusions.

We learn from M. Hébert's writings that the whole group of Eocenes in France and Belgium, below the Thanet Sands, and of which we have no record in England, were deposited in an area alternately sea and dry land, and subject to much oscillation. In the intervals between these recurring changes of level, deposits were formed, but each change denuded them, so that only mere fragments of the original beds now remain. The deposits were formed by a gulf opening into the North Sea which itself communicated with the ocean between the Shetland Isles and Norway. This sea may have penetrated over portions of England and left deposits upon the much eroded Chalk, which denudation subsequently carried away. However this may be, there can be no doubt whatever that the Thanet Sands were littoral deposits of the same sea, and mark a period of depression in the London Basin.

I shall have to speak of the deposits of a Northern and Southern Sea. For the present it will suffice merely to suppose that the English and St. George's Channels were dry land in these remote times, and to distinguish the seas on each side of the barrier respectively as Northern and Southern. Overlying, and perhaps partly contemporaneous with, the shallow sea deposits of the *Thanet Sand* are the English mottled clays belonging to the *Woolwich and Reading Series.* In the direction in which they are deposited and thin out, we have the first evidence of the presence of a mighty river flowing from west to east and scouring and draining a granitic country. In the estuarine beds and the lignites around London, we can trace its delta and embouchure, whilst the marine sands to the east were thrown down by the Northern sea into which it poured. Throughout the whole Eocene time we shall trace the presence of this great river, which receded and advanced and changed the direction of its outfall in obedience to the continued oscillations which this part of its course experienced.

The next movement, at the close of the Woolwich and Reading series, was probably one of slight upheaval, during which the south-east of England became land. Depression followed, and the Northern Sea gradually reoccupied the tract. The shingle and sand banks of the *Oldhaven beds* mark this change, and are the result of the littoral conditions which prevailed

during the recession, but rearranged and also greatly added to by the advance of the sea. The remarkable extent to which the flints are worn shows that the duration of these conditions was very considerable, whilst the change in the fauna seems also to indicate a great lapse of time between these events. In some places rolled pebbles of the mottled clay are included, so that this must at least have been hardened by this time.

In the *London Clay* we see a further depression of the land, enabling the Northern Sea to encroach gradually from the north-east, and ultimately reach as far west as the present borders of Dorsetshire. It never attained more than a few fathoms depth in the western area, and little more than 100 fathoms in the deeper eastern parts. The marine fauna is far more *torrid* looking, owing to a warmer period having set in, produced probably by the rise of land between England and North America which, there is evidence to show, took place about this time, and shut off the cold arctic currents in a manner to be explained further on. The increased temperature of the sea acted upon the land, and enabled a most luxuriant and varied flora to migrate thither; its fruits and seeds are found at Sheppey just as they were stranded and buried in the delta-mud of the great western river.

The river, whose delta has been seen to have been hitherto in the eastern area of the London Basin, seems now to have shifted its course to the south. Its mouth at this period may have been on the since denuded Wealden area, or still further off out at sea.

The *Lower Bagshots* such as we have them at Alum Bay, Studland, Corfe, &c., show no proximity to the sea, but rather the filling in by river action, of a wide valley and shallow lake. The complexity of the stratification renders it doubtful whether two rivers did not unite in this valley, and shows plainly that the waters must in any case have been rapid at times, and subject to periodical fluctuations of volume.

As there are no passage beds or signs of any delta having existed here between the London Clay and Lower Bagshots, we must conclude that the river did not immediately occupy this part of the land left dry by the retreat of the London-Clay sea, but flowed over it long subsequently, gradually shifting its outfall from east to south and west. The great deposits of rolled flints above the London Clay in the eastern parts of the London Basin show that the North-Sea littoral occupied that area for an immense lapse of time. We see by the composition of the western fresh-water beds, that a granite area was still being worn away, and the total absence of flint shows that the western chalk hills were not yet raised into position for denudation by the river, as had they been upraised the course of the river would necessarily have been through them. The absence of lignites in this part

of the series may show the existence of lakes such as that at Bovey Tracy which intercepted drifting timber; its abundance in later beds marks the time when they had been filled in.

In the age of the *Bournemouth Beds* we have the same deposits, but in a more open and level valley, and in close proximity to the sea. In all the lower or freshwater series we have no sign of the presence of sea-water. It is, therefore, not a case of river deposits encroaching on the sea, but of land sinking so gradually as to allow the sea to cover them.

There are absolutely no indications, either in the Lower Bagshot or in the Bournemouth Beds, of the leaves having travelled from afar, and their preservation and local distribution indeed forbid the assumption that they have done so.

In the Bournemouth Beds it is of extreme interest to trace the change from a comparatively upland flora to a valley, and then a swamp flora; to watch the lowering of the land until it became sea; to trace the sea, first trickling in and forming lagoons, then boldly overwhelming their mud deposits with all the imbedded contents, and heaping over them shingle and sand; to see the actual shore line, with land deposits full of plant remains on one side of it, and the bed of the sea with teeth of sharks and rays and shells on the other side.

The elevation of the land towards the west which caused the Northern or London-Clay sea to recede was followed by a depression towards the south-east, which not only diverted the embouchure of the river in that direction, but subsequently enabled the Southern sea, then occupying part of France, to encroach upon the land, and overlap to a small extent the older deposits. In those days the North Sea was completely separated from the Bay of Biscay, neither the English nor St. George's Channels having been formed. Not only are the faunas of the two seas totally distinct, but the lithological characters of the deposits enable them everywhere to be identified.

During the whole of the Eocene period until these beds are reached, we see that the now separate Tertiary Basins of London and Hampshire were continuous, and that identical series of strata were deposited over them without any discontinuity whatever. From this time, however, it is clear, from the total absence of any of the newer Eocene deposits in the former, that a separation had taken place, and that the London basin must in some manner have been severed from the latter. The London area, or at least the ridge which now separates it, must, therefore, have been upheaved at about this time, and perhaps again depressed when the elevation of the Weald took place. It has since been subjected to denudation on a large scale, and it is estimated* that the "vertical loss in the valley of the Thames

* "Memoirs of Geological Survey," iii. 55.

is 500 feet," and that "the flat arch of the Isle of Thanet is not wholly owing to the chalk having been thrown into that form, as at least 600 feet of other formations have been denuded off that tract."

The depression during the *Bracklesham period* was prolonged into that of the *Barton Beds*, which accordingly are found to indicate deeper seas. A remarkable change in the fauna almost suddenly takes place. We have an influx of mollusca of a comparatively temperate aspect, recalling strongly those of the London Clay, and a migration of the more tropical of the Bracklesham forms, only 25 per cent. of the less tropical remaining and mingling with the new fauna. This seems to point to the mingling of the waters of the North and South Seas, and the submergence or destruction of the dividing isthmus, which we know to have been extremely narrow, since deposits of the older seas actually overlapped each other in the Isle of Wight. The succeeding sands belong to the same period, and, as already remarked, do not necessarily indicate change.

After this the seas were again severed, and remained so until recent times, the English Channel, there is reason to believe, having been indeed formed during the human period.

It is interesting to find that Mr. Sorby, now President of the Geological Society, inferred on totally independent grounds—the study of evidence of wave and ripple action in the marine and estuarine beds of the Isle of Wight—that a great river ran from the west into the sea, its estuary including part or the whole of the present Isle of Wight, and that the breadth of its estuary was there 15 or 20 miles. "Yet the dimensions of the river were such as to keep the water in general more or less fresh, and must have been much greater than any now running in England." He was able to trace the approximate size and direction of the shoals, and the axis for the rise and fall of the tides, the direction of the freshwater currents and prevailing winds, at least those which acted with most force upon the sea. This confirmation is of value.

The whole of the vast fluvio-marine or upper Eocene series, 100 feet more in vertical thickness than the height of St. Paul's, presents us with the section of a great river delta formed in an area of depression, similar, perhaps, to that of the Nile or Ganges. Each successive deposit seems to have been thrown down over a more and more circumscribed area, and by a diminishing volume of water. The lowest or Headon Beds alone can be traced across to the main land; whilst the highest Hempstead Beds are almost confined to a single hill. These enormous accumulations present us with an unbroken series of events in one spot, extending from the middle Eocene to the Miocene age in Europe. During this lapse of time we see constant changes and modifications taking

place in the mollusca. Over and over again fresh species of Cerithiadæ, Limnæidæ, Melaniadæ, and other estuarine shells, come in and disappear again. During this period, too, we not only have evidence of the diminution of the volume of the river, but also of a gradual lowering of the temperature, and a more and more wide departure from the tropical conditions which prevailed during the middle Eocene and to the close of the Headon period.

From the close of the Hempstead period until the river finally became lost, its delta or embouchure must have lain in the Solent Channel or in the sea beyond.

The truly marine mollusca of the highest Hempstead bed appear so impoverished and present such a paucity of species and these so stunted, that, coupled with the nature and limited area of the deposit, we must conclude that they were formed in a sea which had shrunk to a mere salt or brackish lake without communication with the open sea beyond. That it was com pletely isolated is certain, since there is no incoming of the Miocene marine forms which existed abundantly elsewhere. The forms are characteristically Eocene, but, except such as the *Corbula* which were enabled to withstand the influx of fresh water, of a degraded type. We have no evidence of the Hampshire Basin again becoming submerged; and although there are abundant indications of larger rivers than those at present existing having flowed through the district even in human times, how far or whether they were in any way connected with the great waters whose history I have endeavoured to trace is very doubtful. Nor have the oscillations which produced these frequent changes of level and at a later period upheaved the Chalk altogether ceased, as there is abundant evidence in the Isle of Wight and on adjacent coasts of elevation and subsidence even within historic times.

Throughout the whole of our Eocene the river action may be more or less distinctly traced, and whether we examine the structure of the rocks themselves or the fossils they contain, the conclusion is irresistibly forced upon us that for countless ages the mouth of a great river occupied at first the south-east and then the south of what is now England; and that during the whole period its delta was subject to change of level, becoming more or less depressed, as we see in recent deltas, permitting now the sea to invade it, now the deposits to rise above water and become inhabited by plants and animals.

Having now shown the existence for countless ages of a river of far greater magnitude than any which the drainage of our present England could furnish, we will briefly consider the probable extent and position of the western land surface from which it flowed.

We are all familiar with that favourite speculation among geologists as to the existence of the former continent of Atlantis. Geologists, zoologists, and botanists have frequently discussed the question; and each time, as fresh discoveries and information have been brought to bear, with more and more weight. The discovery that throughout the whole Eocene period a vast river flowed from the westward, from an area which would now support but comparatively small streams, together with the multitude and variety of the flora and fauna brought down by it, and the former total severance of the North Sea from the Bay of Biscay, reduce from theory to fact, and in the most positive manner, the assumption that a great extension of land then existed to the west of Cornwall. The extraordinary mingling of American, Asiatic, Australian, and African genera in all European floras of Tertiary periods shows no less conclusively that some communication existed between these several lands in former times. In the main, however, I believe the great area of the Atlantic to have been ocean ever since Cretaceous times. We have, in all the Eocene deposits, evidence only of littoral and shallow seas; the Eocene oceanic deposits are unknown, and if ever brought to the surface have again sunk. It is more than probable that most of what was then ocean is, in the main, ocean now; and that vast deposits, similar in extent to the Chalk and older rocks, have been, and are now, uninterruptedly forming. While, however, the wider extent, and perhaps the greater depths, have remained ocean, it is certain that at least the areas contiguous to both America and Europe have been land at no distant date. The contour map of the "Challenger" and other soundings reveal the presence of a great ridge less than 1,000 fathoms deep, running in a zig-zag direction north and south, coming to the surface at the Azores, St. Paul's Rocks, at Ascension, and at Tristan d'Acunha; and soundings taken by the United States sloop "Gettysburg" tend to show that the island of Madeira is also connected with this ridge. An elevation of less than 1,000 fathoms would unite England and Ireland, and extend the land far to the west, 10 degrees beyond Cornwall, producing a surface sufficient to account for all the results we see. An elevation of 2,000 fathoms would give us a continent whose outline can be traced, with elevations 12,000 feet high, and a ridge with a mean height of 6,000 feet, stretching across the equator. If we suppose the depths of the Atlantic on each side of the ridge to have been dry land and former valleys, and there is no inherent impossibility in this supposition, the ridge itself would have an elevation of 15,000 feet, whilst the Ascension, St. Paul, and other peaks would tower to a height of 30,000 feet. We should thus have a chain of mountains above the snow level stretching from an

Australian land across the equator, and forming bridges by which the most temperate plants might have migrated from Australia and from America without being killed by heat. Even with the supposed elevation of 2,000 fathoms the ridge would have been sufficiently elevated to permit the passage of all the plants yet found in Tertiary beds. Without pursuing the subject further, the fact remains that at all events a great tract of land existed where sea is now, and that Cornwall, the Scilly and Channel Isles, Ireland, and Brittany, are the remains of its elevated land. It must, at least, have been as large as France, Switzerland, and Germany, although unconnected with southern Europe. There is, in addition, an ever-increasing mass of botanical and zoological evidence showing that the Atlantic Isles formerly must have been portions of a great continent; and Wollaston, from a study of the insects of the Azores, Madeira, &c., quite recently has been able emphatically to reiterate this fact.

It is probable that throughout the whole of the Eocene period this land was slowly sinking, and opinion has gained ground that the former connection with Australia and Africa became severed during that time. The great submergence, however, did not take place until Miocene times, and was coincident with the elevation of the Alps. The upheaval of the Alps was doubtless directly connected with the sinking of a correspondingly great area, and the magnitude of the subsidence may yet be ascertained by the mass of the elevation. While the London Clay was accumulating, the sea still rolled over some of the loftiest summits of the Alps, and the changes that have subsequently occurred equal in amount the conversion of sea into a continent as large and lofty as that of Europe, Asia, and the north of Africa, and of land into sea to at least an equal extent. "The grand sinking down of the conglomerate of the molasse more than a mile vertically did not begin till all the Eocene movements had terminated, and the upheaval of the same molasse took place at a later period, so that it reached at length its present altitude of 3,000 and 4,000 feet above the sea." * In face of these stupendous movements, the oscillations, whose effects we have endeavoured to trace in England, appear insignificant, the total sum of the depression in Eocene times not exceeding 1,800 feet. Our Eocene area, however, was but the border land, the axis between elevation and subsidence, and, although continually shaken, participated decidedly in neither movement, each tendency being constantly checked.

Vast as must have been the lapse of time during the accumulation of our some 2,000 feet of Eocene strata, still greater appears the interval, of which we have no record in Europe, between the Chalk and Eocene. The almost complete change

* Lyell, "Annual Address to the Geological Society," 1850.

in the fauna and flora, and the utter want of connection between any of the Cretaceous and Tertiary phenomena as seen in England, speak of an almost inconceivable and perfectly immeasurable hiatus. Of the supposed passage beds in Europe many are Eocene, but I believe that in America the gap is at least partly filled up by the Dakota and overlying so-called Cretaceous strata. The true age of these, possessing a molluscan fauna and a flora of strangely Eocene aspect, mingled with Ammonites, Baculites, *Inocerami*, and Dinosaurians, is far older than our Eocene, but certainly newer than our Chalk. My own belief has always been that they are of transition age, and fill up one of the greatest of the geological gaps. The Chalk from its oceanic nature must formerly have been of immense extent, and the patches still extending over wide areas are but relics of it. Throughout ages preceding the Eocene time and ever since until the present day it has been incessantly worn and denuded, to what extent we imperfectly see in the enormous beds of shingle and sand in the Eocenes, Pliocenes, and Pleistocenes, and in the gravels of the present sea bed.

REVIEWS.

MR. PROCTOR'S WORKS.*

ANY subject on which Mr. Proctor undertakes to write is sure to be treated in an eminently lucid and concise manner. Indeed the popularity of his printed works, and of his admirable scientific lectures, affords sufficient evidence that the fact is fully and widely appreciated. Setting apart the "Star Atlas" as more topographical in character than literary, but which, already in its fourth edition, is perhaps the most convenient guide for the amateur star-gazer as yet published, all the works enumerated below, with one exception, have run through two or more editions.

"The Plurality of Worlds," of which the fourth reprint has lately appeared, originally propounded views differing from those usually accepted, these being based on the signs of systematic aggregation among the lucid stars, and of a more intimate association of those stars with the Milky Way than could be expected were Sir W. Herschel's fundamental theory correct. In the last edition, besides other modifications, three important discoveries, that namely of oxygen in the sun, of the fading of the new star in Cygnus into a planetary nebula, and of the two moons of Mars, have been incorporated as "supplying new and strong arguments in favour of the theory of cosmic evolution advanced."

The third edition of the "Transits of Venus" has been enriched with the results of the British Expedition, which if not altogether so satisfactory as could be wished, so far at least as their separate examination is concerned, serve at least to dispose finally of all doubt on the questions which were discussed before the transit of December 9, 1874. The memorable controversy on this subject will be in everyone's recollection, and probably Mr. Proctor is correct in stating that "it is altogether unlikely that any

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* "Other Worlds than Ours: The plurality of worlds studied under the light of recent researches." Fourth Edition. "A New Star Atlas." Fourth Edition, folio. "Transits of Venus: A popular account of past and coming transits from A.D. 1639 to A.D. 2012." Third Edition, to which is added a note on the results obtained by the British Expedition. "The Universe of Stars: Presenting researches and new views respecting the constitution of the Heavens." Second Edition. "A Treatise on the Cycloid and all forms of Cycloidal Curves, &c." By R. A. Proctor, B.A., Scholar of St. John's Coll., Cambridge, Mathematical Scholar and Hon. Fellow of King's College, London. 8vo. London: Longmans, 1878.

attempt will be made hereafter to make Delisle's method the basis of any systematic series of transit operations."

"The Universe of Stars," now in its second edition, is dedicated to Adams and Leverrier, and aims at establishing a new theory of the universe, by which, "instead of separating the stars and nebulæ into distinct systems, or rather of looking on the stellar system as a member of the system of nebulæ, we seem compelled to look on almost every object visible even in the most powerful telescope as a portion of one system, which comprises within its range single, multiple, and clustering stars, irresolvable nebulæ, gaseous bodies of symmetrical and unsymmetrical figure, and in all probability myriads of other forms of matter as yet undetected." It gives an accurate account of the results obtained by Sir W. Herschel during his long and arduous labours in the study of the star-depths. The conclusions arrived at are briefly that "the sidereal system is altogether more complicated, more varied in structure, than has hitherto been supposed. Within one and the same region coexist stars of many orders of real magnitude, the greatest being thousands of times larger than the least. All the nebulæ hitherto discovered, whether gaseous or stellar, irregular, planetary, ring-formed, or elliptic, exist within the limits of the sidereal system. They all form part and parcel of that wonderful system whose nearer and brighter parts constitute the glory of our nocturnal heavens." It is impossible, with a theme like this, for a thoughtful mind not to be led on to much grand and far-reaching speculation. In the presence of the Three that bear witness to man's puniness, Matter, Space, and Motion, once more the cry goes up to heaven so well formulated by Newton, as of a child picking up pebbles of knowledge on the sea-shore, while the great ocean of truth lies all undiscovered before him. In this book it takes the form of a quotation from Jean Paul which singularly reproduces a kindred dream of the boldest and most imaginative thinker of antiquity, namely the myth of the Whorls at the end of Plato's "Republic."

The "Treatise on Cycloids and Cycloidal Curves" stands on a somewhat different footing from the works above named. Whereas they are speculative and theoretical, this is precise and constructive. It deals primarily with the geometry of cycloids, curves traced out by a point in a circle rolling on a straight line, or on or within another circle, and trochoids, traced out by a point within or without a circle so rolling.

The cycloid, long known to ancient astronomers, was first fully investigated by Galileo. By suitably selecting radii and velocities, every form of epicyclic curve can be obtained, including the epicycloid and the hypocycloid. When the radius of the fixed circle is indefinitely enlarged, or in other words, when the centre of the moving circle advances uniformly in a straight line, the curve traced out by the moving point becomes a trochoid, and may either be *prolate*, *right*, or *curtate*, according as the velocity of the moving centre is greater, equal to, or less than the velocity of the point around that centre. Lastly, if the radius of the moving circle is indefinitely enlarged, so that a straight line is carried uniformly round a centre while a point travels uniformly along the line, the curve traced out becomes a spiral of the family to which belong the spiral of Archimedes and the involute of the circle.

Ancient geometers were not very successful in investigating these curves. "Even Galileo was reduced to the necessity of weighing paper figures of the

curve in order to determine its area. Pascal seems to have regarded their relations as of sufficient difficulty to be selected for his famous challenge to mathematicians to try whether a priest who had long given up the study of mathematics was not a match for mathematicians at their own weapons."

The book itself is divided into seven sections, dealing successively with the right cycloid, the epicycloid, and hypocycloid, trochoids with an appendix or elliptical hypotrochoids, the trisectrix, the spiral of Archimedes, and the spiral track of a planet's shadow in space, motion in cycloidal curves, epicyclics, equations to cycloidal curves, and the graphical use of cycloidal curves to determine the motion of planets and comets, and of matter projected from the sun.

Regarding the treatment of these different topics it is impossible within the space of a notice such as this to give more than a few general indications, although the writer, contrary to the supposed practice of reviewers, admits to having read with interest the 250 pages of which the work consists from title-page to colophon.

In the first place it is a refreshing study in geometry as opposed to analysis. There are many minds, especially those of the mechanical type, to which the former gives more objective reality than the latter; and there are others to whom deltas and sigmas are a sealed language, but who can nearly always puzzle out a construction and its consequent demonstration. Secondly, the subject, somewhat spinous from outside, really contains much interesting matter, and touches on practical points more often than at first appears. Examples of this are demonstrations that the area of the cycloid and its base is three times that of the generating circle; that the evolute of the cycloid is an equal cycloid—resumed more fully in a later chapter—a property utilized in Huyghens's ingenious though unsuccessful cycloidal cheeks for producing isochronism in pendulums; that the hypocycloid becomes a straight line when the diameter of the rolling circle is equal to the radius of the fixed circle, as seen in "sun and planet wheel" mechanisms; the description of the cardioid or epicycloid traced by a point on the circumference of a circle rolling on an equal circle; of the involute of the circle regarded as an epicycloid whose generating circle has infinite radius; that the hypotrochoid becomes an ellipse when the diameter of the rolling circle is equal to the radius of the fixed; the few lines about the trisectrix, and its manner of solving the much vexed problem of the trisection of an angle, and the proof of the identity of the epitrochoid with the spiral of Archimedes.

The comments on planetary and lunar epicycles, with two beautiful plates in illustration, deserve special mention; as also do those on which Lescarbault's observations of the supposed intra-Mercurial planet Vulcan turned. The note on Mr. Perigal's geometric chuck, by which many of the illustrations have been furnished, will recall an old friend to many readers. The companion to the cycloid, better known as Roberval's "curve of sines," has the deepest interest to students of acoustics, vibration, or simple harmonic motion in any form; and the graphical use of the cycloid opens a comparatively fresh field of labour.

Speaking generally, the writer has derived so much pleasure from a perusal of this little book that he confidently recommends it to his brother students. Mr. Proctor would confer a boon on many mechanics and physicists if he

were to supplement this volume by undertaking a simple description of other higher curves now but little known and concealed under mystical titles such as "the Witch," with methods for their continuous instrumental tracing where possible, and a geometrical delineation of their properties.

W. H. STONE.

THE SYSTEM OF THE WORLD.*

NEWTON stated the first law of motion in the following words: "Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum." And since his time it has been universally accepted that inertia was equally applicable to uniform unretarded motion in a straight line as to a state of rest.

Mr. Jordan, however, states in his first lecture that "the latter half of the definition of *vis inertiæ* is, without the slightest fear of anyone in this room rising to contradict what I say, a mere fiction. It is a fiction, and has always been a fiction. It is a fiction invented to explain phenomena for which no other explanation had been found; no practical evidence of its existence can be given in any physical phenomena."

It is sincerely to be hoped that the writer of this passage will not try the dangerous, though perfectly conclusive experiment, of jumping out of a railway train when in motion. He might obtain unpleasant "practical evidence of its existence."

In place of this "fiction" he seems to substitute what he terms "astral gravitation." This new force he holds to be demonstrated by Dr. Wallich's discovery of animal life at the bottom of the sea, and Mr. Rainey's experiments as to molecular motions of small particles in hermetically sealed globes of water.

The writer of the present notice is well acquainted with these laborious and remarkable observations of Mr. Rainey, but has always regretted that their author should have attributed the really existent currents to so novel and unheard-of a cause as "an alteration in the centre of gravity of the earth." Such a phenomenon, if existent, would surely be known to astronomers, and would probably appear in the Nautical Almanac.

The lecturer then proceeds to describe and explain ocean-currents, of which a good map is appended, on his own hypothesis; especially controverting Dr. Carpenter's views on the subject, and the usual temperature theory.

In a second lecture, after adverting to "the fickleness of common sense," the trade-winds are referred to "astral gravitation," as are also ocean currents "analogous to the trade-winds." Dr. Carpenter and Sir Wyville Thomson are stated to have "strangely mistaken sloppy mud for living flesh in the form of Bathybius," and thus to have "made the greatest mistake that has ever been recorded in the annals of science."

The "new theory of vis inertiæ" is next applied to the tides, and

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* "The System of the World. Challenge Lectures. Winds, Ocean Currents, and Tides, and what they tell of the system of the world, &c.," by William Leighton Jordan, F.R.G.S. Second Edition. Hardwicke. 1877.

"Captain Evans, Hydrographer to Her Majesty," is told that "considering the trouble I have taken year after year to induce him to give some attention to the subject, and that now he tells me that he has not been influenced by my writings, I think it is his duty to state what is the cosmical cause, if not that indicated by the theory of vis inertiæ." The said theory is then applied to the correction of Kepler's law, and to demonstrate the falsity of the "linea recta" axiom in Newton's law. "This I claim to have done," says our author. "I have proved it false by showing the new theory of vis inertiæ, which is incompatible with that so-called axiom, to be true."

After each lecture, which seems to have elicited "loud applause," cries of "hear, hear," and occasionally of "Shame! No, no," "members or ex-members of the Council of the Royal Society and the scientific staff of the Challenger Expedition" were "specially invited to discuss what was said by the lecturer."

This they strangely and unaccountably neglected to do. Perhaps it would be best to follow their example.

W. H. Stone.

LLOYD'S PHYSICAL PAPERS.*

An honoured name in the annals of science is that of Humphrey Lloyd; none the less so because it takes back the younger votaries of science to an almost prehistoric period. "There were giants in those days," and it is subject for sincere joy and heartfelt congratulation that one or two still survive, not only in physical but in intellectual vigour.

Malus, Fresnel, Baden-Powell, Wheatstone, Barlow, and Pereira have passed away, not without leaving their mark on physical science; and the links which connect them with modern acquisitions are well represented in the present volume. Indeed it is no small service conferred on science when a veteran in the army that makes war upon nature condescends to collect for his subalterns and juniors a record of his fights. Each paper in this book is like a clasp on the medal of "a Peninsular." "Orthes, Nivelles, Badajoz, Torres Vedras," bear no distant relation to "Report on the Present State of Physical Optics," 1834; "Observations of the Direction and Intensity of the Terrestrial Magnetic Force in Ireland;" "On the Determination of the Horizontal Intensity of the Earth's Magnetic Force in absolute measure;" "Notes on the Meteorology of Ireland" and "On the Rise and Progress of Mechanical Philosophy: Introductory Lecture delivered in the Philosophy School of Trinity College in Hilary Term, 1834."

The great defect of physics as a study is the difficulty of knowing what has been done before. Hence immense waste of work in reinvestigation and rediscovery. "The older writers on physics were sad plagiarists of our new discoveries," saith the genial and patient teacher of physics at much-abused South Kensington. But with more retrospective compilations like this and the similar work of Sir William Thomson, many a wasted hour might be

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* "Miscellaneous Papers connected with Physical Science." By Humphrey Lloyd, D.D., D.C.L., Provost of Trinity College Dublin, formerly Professor of Natural Philosophy in the University. Reprinted from several Reports and Transactions. London: Longmans. 1877.

saved, many a disappointment might be averted, and many an error in the profit and loss account of science on ground of double entry might be remedied.

FLOWERS.*

IN Botany, as in Zoology, since the publication of Mr. Darwin's "Origin of Species" an entirely new start has been made. The influence exerted by this and the other works of that distinguished naturalist does not depend in the very least upon the acceptance or non-acceptance of his peculiar theory, or indeed of the doctrine of evolution in any form; it is founded upon the fact that the mere raising of the question by a voice so powerful, opened up perfectly new lines of investigation for the naturalist, and drove him perforce to the consideration of those interpretations of organic beings as living things, which there was great danger might be lost sight of under the teachings of the purely anatomical and analytical school of naturalists that was rapidly gaining the upper hand. Mr. Darwin's works, whatever might be their intrinsic merits or demerits, certainly had this beneficial effect—they drove naturalists to the study of organisms in nature, of their mutual relations and influence on each other; they gave a new and striking significance to the facts of geographical distribution, and invested the facts of palæontology with an interest which they never before possessed. Naturally also the old teleological view, according to which, as a general rule, natural objects were looked at chiefly as they affected ourselves, speedily fell into disrepute, and a grander and far more healthy philosophy of nature was insinuated into the minds of men. We began to realize that animals and plants might, perhaps, exist for themselves and for each other without reference to the convenience or enjoyment of the "Lords of the Creation," and as this notion gained ground, a multitude of harmonies, before unsuspected, were rapidly brought to light.

Nowhere have the consequences of the new methods of study been more striking than in the domain of flowers. A multitude of flowers which seemed to ordinary observers mere meaningless *bizarreries*, and puzzled the botanist by their peculiarities of structure, were found, as in the case of the orchids, to have a very serious significance, and their interpretation led indirectly to a comprehension of an immense number of other instances of apparent anomalies in floral structure, which were now seen to have a deep meaning and to be in accordance with a very general law. All these observations, by guiding to an appreciation of the influence exercised by insects upon the fertilization of flowers, opened a fresh field of research as to the purpose in nature of the bright colours and the odours of flowers, and led to the investigation of a host of phenomena which had previously attracted little attention.

It is not our purpose, nor is it within our power in the space at our command, to attempt giving even a rough sketch of the infinity of points of interest connected with flowers and flowering plants of which the new

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* "Flowers; their Origin, Shapes, Perfumes, and Colour." By J. E. Taylor, Ph.D., F.L.S., F.G.S. Small 8vo. London: Hardwicke & Bogue. 1878.

philosophy of natural history has opened a prospect, and which Dr. Taylor, in the little book before us, has very successfully discussed. Here the reader will find explained, and in a pleasant and popular style, all the points to which we have merely alluded above, and a good many more, the book being in fact a brief treatise on flowering plants in themselves and in their relations to the outside world. The geographical distribution of plants not unnaturally occupies a prominent position, and in connexion with this their geological history is of importance, besides leading to certain curious speculations with regard to the relations prevailing in past time between insects and plants. The remarkable results of the investigations of Mr. Darwin and others upon the orchids and upon dimorphic and trimorphic plants are given in considerable detail—in fact the whole subject is well treated, and the author may certainly congratulate himself on having fulfilled the design with which he set out, namely, "to place before that portion of the intelligent public who have the desire, but neither the time nor the opportunity, to make themselves acquainted with natural science, the charming and suggestive results of modern botanical investigation." It is, indeed, for its suggestiveness, in addition to the vast amount of information collected in its pages, that we should specially recommend Dr. Taylor's little book; it will probably suggest to many members of "the intelligent public" who *fancy* that they have no time or opportunity for natural-history studies, that after all here are paths indicated which may lead them to a keener appreciation of nature, and of the system of nature, by means of personal observation, without making any very serious inroads upon their time; and the book is so many-sided that all fancies may be hit by it.

The little volume is illustrated with eight coloured plates of common flowers, each containing four figures, and with a great number of very good wood engravings scattered through the text.

FERNS.*

WE are told by an excellent authority, of a certain Peter Bell, that

> "A primrose by the river's brim
> A yellow primrose was to him,
> And it was nothing more."

His opinion of ferns would probably have been of a still more disparaging nature; and we are afraid that in this respect there are a good many Peter Bells in the world. It is, so far as we know, the main object of Mr. F. G. Heath's literary activity to convert these benighted individuals to what, with all the zeal of a missionary to the heathen, he regards implicitly as the true faith, namely, a belief that his favourite ferns are among the most beautiful of plants; and truly those who are familiar with them will hardly be inclined to gainsay this opinion. In their graceful curves, in the delicate tracery of their fronds, in the beautiful effects of colour and of light and shade which they present, none of the lowlier growing plants come near them; and it is

* "The Fern Paradise, a Plea for the Culture of Ferns." By Francis George Heath. Fourth Edition. 8vo. London: Sampson Low & Co. 1878.

probably owing chiefly to their want of flowers that they are indebted for that amount of non-appreciation which they certainly do meet with.

Certainly the ferns are fortunate in their present advocate. Mr. Heath writes about them with a knowledge derived from years of study, and with an enthusiasm which ought to carry his readers with him. In his "Fern Paradise," a fourth copiously illustrated edition of which has just appeared, his descriptions of ferny scenery, especially in Devonshire, the land of ferns, suffice to inspire the reader with a longing desire to join in the shady walks and pleasant rambles which are so vividly presented to his imagination, and gain infinitely by their contrast with the brick walls and glaring pavements among which it is the fate of so many of us to pass the greater part of our lives. This portion of Mr. Heath's book is beautifully illustrated with numerous photographic views of the scenery described, reproduced by the Woodbury process. These views have been selected with the eye of an artist.

Mr. Heath's task does not end here, however. The missionary's business would be but lamely performed if he merely convinced his converts of the existence of Paradise without teaching them how to get there; and our author's mission would be equally unfulfilled if he stopped short at merely convincing his reader's mind that ferns are very beautiful things. Mr. Heath's conviction is that the contemplation of the beautiful forms and delicate colours of ferns, the taking an interest in them and learning something about them, cannot but have a refining and improving influence upon the mind, and that the cultivation of ferns may in this way be most beneficial. He further shows that this cultivation may be very easily effected, so that those to whom a country ramble comes like a brief ray of sunshine in an otherwise clouded existence may surround themselves at home with the beautiful feathery forms of these children of the woods and green lanes. Some of them may be grown in almost any scrap of garden, in houses, windows, yards, and even in the areas of city dwellings. Lace and jewels, fine furniture, pictures, statues, and vases, are all beautiful things, but to get them is the lot of but few; the "Fern Paradise," on the contrary, may be realized by almost everyone and in almost any situation. As a guide to such of his readers as may desire to put his teachings to practical use, Mr. Heath gives a series of descriptions of British ferns, free as far as possible from scientific technicalities, and accompanied by statements as to the habits and needs of the various species. This section of the book will be very useful to the beginner, whose determinations of the species will be materially facilitated by the figures, showing the ferns in white upon a black ground, with which it is illustrated.

While we cannot go quite so far as our author in his belief in the ameliorating influences of fern-culture, we can sympathize with him in his desire to see these beautiful plants more generally cultivated. The mere tending of them and watching their growth must be a source of pure pleasure to those who can be induced to take an interest in them; the contemplation of their beautiful forms and delicate colour must also be a source of gratification; and unquestionably the influence of such pleasures upon the mind must be beneficial, even independently of any knowledge of natural phenomena which may be picked up by the way. We can recommend Mr. Heath's

book as one that will be read with much interest, and in conclusion can only express our gratification that its success has already been such as to warrant his expanding his original little volume to its present form, in which it is what our French friends would call a veritable *ouvrage de luxe*.

NICHOLSON'S MANUAL OF ZOOLOGY.*

WE have much pleasure in calling attention to the publication of a fifth edition of this excellent manual. It is, perhaps, the best exponent of modern non-Haeckelian views on zoological classification that we possess in English; and in the present edition, which is considerably enlarged, the author has been enabled to insert a good many more details as to the habits of animals and their relations to each other than were to be found in former issues, which might be regarded rather as giving the dry bones of animal morphology, than as expositions of the science of animals as they "live and move and have their being." Much room for improvement in this respect no doubt still remains, but unless the book could be at least doubled in size, it would hardly be possible to give a satisfactory zoology embracing as much of structural detail as the present work. As a manual for students, which is what it professes to be, Professor Nicholson's book is exceedingly good.

This fifth edition does not differ in any important particulars from its predecessors, as regards the general treatment of the subject. The classification adopted is the same, with some slight exceptions, where groups of recent establishment have been accepted; but the treatment is much fuller, and the results of very recent researches have been worked in, the additions being even more extensive than would appear from the increased number of pages, seeing that the author has adopted the device of printing a good deal of matter of subordinate importance in small-type paragraphs.

Another new feature in the book is the suppression of the synoptical tables of families which appeared in former editions and the substitution for them of class bibliographies, which, if in some instances rather defective, will nevertheless prove useful to the student by indicating the main directions in which his further studies may be profitably pursued. This edition, which has many new illustrations, worthily maintains the reputation acquired by its predecessors.

THE MAMMOTH AND MAN.†

THE object of the writer is to prove that there are no traces of Man having existed before or during the so-called Glacial Period—that this Period occurred at no considerable distance of time ago—and that Man was intro-

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* "A Manual of Zoology for the Use of Students, with a General Introduction on the Principles of Zoology." By Henry Alleyne Nicholson, M.D., D.Sc., M.A., Ph.D., F.R.S.E., F.G.S. Fifth Edition. 8vo. Edinburgh and London: Blackwood & Sons. 1878.

† "The Epoch of the Mammoth and the Apparition of Man upon the Earth." By J. C. Southall, A.M., LL.D. With Illustrations. 8vo. Trübner & Co., London: 1878.

duced recently, since that Period, in the fulness of his development. Dr. Southall avows his desire to treat of *facts* in discussing the question of Man's age on the earth; but he has apparently a very limited view of many of the facts he refers to, and certainly uses them only as data for special pleading.

The style of argument adopted can be thus illustrated. No human remains have been found in Miocene or Pliocene strata, *ergo*, Man never existed in Tertiary times! High civilization obtained in the earliest known times of Egyptian and Babylonian history, *ergo*, there was never a purely Stone Age in Egypt and Chaldea! Cain *built a city*, *ergo*, the earliest men were architects! Chaldeans and Egyptians appear to Dr. Southall to have come abruptly on the scene in a civilized condition some 6,000 or 10,000 years ago, *ergo*, Palæolithic Man can claim no higher antiquity; hence, too, there was no *evolution* as far as the human race is concerned! Further, many beliefs, customs, and modes of worship are, or have been, common to widely separate nations, *ergo*, mankind came at once, Egyptian, Hindoo, Babylonian, the "fossil man of Mentone," the Neanderthal savage, and men of the Somme Valley, and not more than 10,000 years ago! Errors have been made by antiquaries and others in estimating the age of antiquities, fossil bones, marks on bones, and of natural and artificial accumulations, *ergo*, there is no acceptable evidence of high antiquity for man!

Megalithic Monuments, Lake-dwellings, Danish Shell-mounds, Peat-deposits, Bone-caves, and River-gravels, are all regarded as having led both original and subsequent observers over treacherous grounds into fallacious reasonings, which our author thinks himself competent to make good! Because there are comparatively recent stone-implements, pile-dwellings, refuse-heaps, and peat-beds, none can be credited with a high antiquity! Because floods can shift gravel and other débris, a grand pluvial flood must have followed the Glacial period and arranged the valley-gravels of Western Europe! Because Assyrian kings hunted the Asiatic elephant, therefore the *Elephas primigenius* of Northern Europe, or Siberian Mammoth, carries no evidence of high antiquity! Oscillations of land and sea are allowed to have taken place on a grand scale; but, as one of the evidences "that *force* as well as *time* is an element in geological action," the long-exploded mistake about Jorullo is reproduced; and the volcanic phenomena of Miocene and Pliocene times are brought in as "recent geological convulsions"; but how they fall in with our author's notions of the history of man is not clear. And thus we go on reading Dr. Southall's many notes on other people's discoveries and writings, over many parts of the globe, and find that the evidences for man's great antiquity are carefully eschewed, and whatever can be brought forward to strengthen the writer's own opinion is industriously presented. When we look for a scientific treatment of such subjects as the alterations of level by oscillation and denudation—of successive depositions and removals of material—of changes in the configuration of Western Europe, where certain races of early man did exist—and of variations of climate, and consequent differences in successive faunas and floras, we have the results of imperfect and one-sided efforts to master the facts and opinions which geologists and naturalists have given to the thinking world.

THE HUMAN EYE.*

IN this little book Dr. Dudgeon furnishes his readers with an excellent popular description of the eye, and of the optical principles by which it is enabled to convey to the brain the visual impressions of external objects. The whole explanation is very clear and lucid, and the author has availed himself of all the more important recent treatises on vision published both in this country and on the Continent. Of especial interest are his observations on sub-aquatic vision, upon which he has made many personal experiments which lead him to the conclusion that perfect vision cannot be attained under water by the unassisted eye, and he accordingly recommends the use of air-spectacles, the construction of which is fully explained, in order to counteract the altered conditions of refraction which must affect vision under water.

This, however, is not the only original part of Dr. Dudgeon's little book. The question of the means by which the eye is enabled to adapt itself to vision at different distances is one which has always presented considerable difficulties, and the observed phenomena, and especially the direct observation of the form of the surfaces of the lens in near and distant vision, led the author to the conclusion that changes of convexity in that particular part of the eye were not the causes of the adaptation of the eye. Moreover the mechanism for producing them seemed to be wanting. The consideration of the phenomena presented by the surface of the lens, when the eye is directly examined under changed conditions of vision, suggested to Dr. Dudgeon another explanation, namely, a shifting of the position or twisting of the lens relatively to the axis of the eye, which, he maintains, from the form of the two surfaces of the lens, would produce the alterations of refractive power necessary to adjust the eye to changes of distance. Upon this ingenious hypothesis we shall not venture to offer an opinion, but it certainly seems worthy the attention of students. The little book is fully illustrated with woodcuts, and is a valuable contribution to our popular scientific literature.

A GEOLOGICAL MAP OF LONDON.†

THIS is Stanford's capital "Library Map of London," coloured geologically. The scale is six inches to a mile, far larger than that of any other geological map of London, the Geological Survey having published only the ordinary "one inch" map. The area embraces on the north Hampstead, Highgate, Clapton, and Leyton; on the east Stratford, Greenwich, and Lee; on the south Beckenham, Anerley, and Merton; and on the west Wimbledon, Putney, Hammersmith, and Willesden.

The compilation of the geological information has been carefully carried out by Mr. Jordan, who has great experience in such work. The map shows not only the Chalk and the various Tertiary beds, but also, to quote

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* "The Human Eye: its Optical Construction popularly Explained." By R. E. Dudgeon, M.D. Sm. 8vo. London: Hardwicke & Bogue, 1878.

† Stanford's "Geological Map of London. The geology compiled from the Maps, &c., of the Geological Survey by J. B. Jordan" London, Stanford. 1878.

from the Publishers' Circular, "the superficial deposits of gravel, &c., which, though comparatively thin, have so great an effect on the form of the surface and the nature of the soil."

The work can be had in various forms, to suit the convenience of purchasers. Each of the 24 sheets is sold separately; or the whole in a portfolio, with an index inside the cover; or mounted together as a large wall-map.

BIBLE PLANTS.*

BIBLE students have every reason to be grateful to Mr. Smith, the ex-curator of the Botanical Gardens at Kew, for the concise history of the plants mentioned in the Bible which he has just prepared for their delectation and improvement. He has devoted a considerable amount of labour to the identification of the plants and plant-products mentioned in the Scriptures, and to collecting and considering the opinions of previous writers, and in this little volume we find the results of his investigations. The whole number of plants, as he tells us, is only about 100, and those who have never paid attention to the subject will be rather surprised to find how many plants, called by familiar English names in our "Authorised Version," are anything but what they profess to be. Thus the "tares" spoken of frequently in the Bible are not the same as the wholesome and nutritive vetch which is known by that name, but a species of grass very similar in appearance to wheat or rye, but the grain of which is so poisonous that serious illness or even death has been caused by eating bread containing its flour. This explains some of the references to tares, and especially to the sowing of tares amongst an enemy's wheat. Many points of equal interest to this appear throughout the book, which is illustrated with ten characteristic plates. We recommend Mr. Smith's little volume to the attention of all interested in Bible studies, especially to those who have to do with the communication of information on scriptural matters to the young.

PHOTOGRAPHIC SPECTRA.†

THIS is an excellent work of reference, well got up, and containing thirty-eight excellent autotype plates of spectra, including an additional photograph of the solar spectrum and spark in air, compared for the purpose of showing the position of bright lines in the former assigned by Professor Draper to oxygen. Other bright lines are seen towards the green end in both spectra, which are not included in Professor Draper's photograph.

The metallic spectra were obtained by the induction spark and by means of the electric arc. The wave-lengths are taken partly from Lecoq de Boisbaudran, partly from Dr. Watt's "Index of Spectra." The photographs are printed four to a page, on uniform lengths of four inches, on prepared paper of fine nacreous surface, which would probably lend itself well to accurate measurement.

* "Bible Plants: their History, with a Review of the Opinions of Various Writers regarding their Identification." By John Smith, A.L.S. 8vo. London: Hardwicke and Bogue, 1878.

† "Photographic Spectra." One hundred and thirty-six photographs of metallic, gaseous, and other spectra, printed by the permanent autotype process with introduction, description of plates, index, &c. By J. Paul Capron, F.R.A.S. 8vo. London: Spon. 1877.

SCIENTIFIC SUMMARY.

ANTHROPOLOGY.

North American Inscribed Stones.—In a burial mound on Cook Farm near Davenport, Iowa, the Rev. J. Gass has found some inscribed tablets of bituminous shale, which, if they prove genuine, of which we must confess there seems to be some doubt, will take their place among the most interesting prehistoric pieces of human workmanship. The mound under exploration contained two grave-hollows, in one of which, at a depth of about five and a-half feet from the surface there were three skeletons, two of adults, with the third (that of a child) lying between them; with these were found copper beads and copper axes, besides other objects. The second grave-hollow contained some fragments of skull-bones, vertebræ, &c., and the inscribed tablets above-mentioned. Mr. Gass describes the mode of their occurrence as follows:—"The two tablets were lying close together on the hard clay, in the north-west corner of the grave, about five and a-half feet from the surface of the mound. . . . The smaller one is engraved on one side only, and the larger on both sides. . . . Both were closely encircled by a single row of limestones. They were covered on both sides with clay, on removal of which the markings were for the first time discovered. A number of fragments of the coal-slate lay in the immediate vicinity of the tablets. It should also be remarked that I did not leave the mound after penetrating through the frost* until the tablets were discovered and taken from their resting-place with my own hands."

These tablets are described and discussed at considerable length by Dr. J. Farquharson, whose account of them is illustrated with reproductions of photographs. Dr. Farquharson also notices the various inscribed stones which have been previously found in the United States, most of which appear to have been, if not frauds, at any rate not genuine; the best of them is that of Dighton Rock near the mouth of Taunton River in Massachusetts, which is a Runic inscription recording the occupation of that region by 151 Northmen with Thorfins, who is identified with Thorfinn Karlsefn, with whose saga it closely agrees.

The smaller of the two tablets is called by Dr. Farquharson a "Calendar-

* The excavation was made on the 10th of January, when the ground was frozen hard. A shell-layer traversing the mound above the graves is said to have been undisturbed.

stone." It is marked with a central circle, having a radius of one inch, round which, with the same centre, are inscribed three other circles at an average distance of three-quarters of an inch. The space between the first and second circles is marked with four characters dividing it into quadrants; in the space between the third and fourth circles there are twelve characters at equal distances apart. As to the signification of these characters, Dr. Farquharson is quite in the dark; some of them have a rude resemblance to animals, and one is interpreted as representing cross-bones surmounted by a flame, but unaccompanied by the skull which is always associated with them in Maya sculptures. The Doctor suggests a zodiacal interpretation of this design, but does not seem to place much faith in it himself.

One side of the larger tablet presents us with a "sacrificial or cremation-scene," showing a number of human figures with their hands joined as forming part of a great ring round a fire, with several human bodies lying on the ground as if ready to be consigned to the flames. This is in accordance with the account long since given by La Hontan, who says:—"The savages on the Long River (Mississippi) burn their dead, reserving the bodies until there are a sufficient number to burn together, which is performed out of the village, in a place set apart for the purpose." Above the cremation-scene are three segments of large circles, forming a rainbow-like arch stretching from side to side of the stone; the space below this arch, which may be taken to represent the sky, is thickly studded with small points, and bears two circular figures one on each side, the whole no doubt intended to represent stars, sun, and moon. The spaces between the curved lines, and the space above them, are entirely covered with engraved characters, of which no one has yet discovered the nature. In all, there are ninety-eight characters, but twenty-four of these are repetitions, so that there are seventy-four distinct figures. As a whole, or even generally, they resemble no known alphabet, although, of course, it is not difficult to trace a similarity between some of the characters and those of certain alphabets.

The engraving on the other side of the large tablet is explained as a "Hunting Scene." Thirty animals are rudely represented upon it, and among them are some which Dr. Farquharson interprets as mastodons, and it must be admitted that they have a very elephantine outline, although neither the trunk nor the tusks are shown. This picture is not accompanied by any characters. It is evident that if the genuineness of these articles can be fully established their discovery opens up an entirely new view of the condition of man in America, where, hitherto, he is supposed, at any rate in primitive times, not to have advanced beyond the art of picture-writing. —(*Proceedings of the Davenport Academy of Natural Sciences*, vol. ii., part i.)

A Prehistoric Foundry at Bologna.—M. E. Desor has read to the Society of Natural Sciences of Neuchâtel, an interesting memoir upon some discoveries of prehistoric remains lately made in and near Bologna. He says the evidences of prehistoric Italian civilization lately obtained from diggings in that district (at Villanova, Bonzano, Arnoaldi, Benacci, and Luca), notwithstanding a resemblance to the true Etruscan type, have, in many respects, a definite special character ("type of Villanova"). As it was impossible not to see what a near relationship unites the art-products found about the banks of the Reno

with those of the Swiss lake-stations of the bronze-age, and with those of the Gallic of the commencement of the iron age, and as even a portion of the local differences lose in importance from the circumstance that the one series of objects was specially destined for the service of the living, and the other for the adornment of the dead, the supposition that they had a common origin was easily arrived at. Their common source was supposed either to be among the comparatively more advanced peoples of the plain of the Po, or, as no place for their production *en gros* has yet been found there, in the old seat of the Bronze-civilization at the foot of the Caucasus. About a year ago, in the middle of Bologna, near the church of St. Francis, M. Zannoni, an engineer, came upon a great earthen vessel, 4 ft. 8 in. high and 4 ft. broad at a depth of about two feet. In this there were 14,000 bronze articles of admirable workmanship, packed together as closely as possible, and all coated with the ordinary green patina. MM. Desor and Berthoud came to Bologna just when the whole contents of this vessel were exhibited in the Town Hall. Of celts there were 2,077, from rough, wedge-shaped ones to the most highly finished and engraved specimens; 2,407 fibulæ in 25 principal forms; a great number of knives of every form and size, and also in part engraved; hundreds of chisels and a good many gouges; 275 lance-heads varying greatly in length up to more than a foot, although remarkably similar in form; half-a-hundred sickles, many of them very large. Of bits for horses there were about 60, made evidently for a considerably larger race than that of the Swiss Lake-stations. There were fish-hooks, harpoons, hammers, a large anvil, a plane, and a dozen saws. Further, several hundred different bracelets, all massive, with the ends often in the shape of the heads of animals; numerous hair pins, although comparatively fewer than in Switzerland, and usually with flat heads; 50 razors with small handles; numerous ornamented plates, the pattern of which in part resembled those of Villanova; a comb; short tubular pieces with appendages like ear-drops; quadrangular plates with markings, regarded by Gozzadini as little bells. The weapons were comparatively few, and consisted of daggers, arrowheads, and swords. With all these were casting moulds both of hard bronze and of earth.

The preservation of the specimens was not quite uniform. Among the hatchets there are some quite rough, still showing the line of junction of the moulds; others worn, and others quite new. Of the fibulæ also, some are injured and others quite perfect; some have the tongue damaged or lost; in others it has been refixed with a small iron rivet, or completed by a small lamina of bronze of suitable form. If to all this we add, that fragments and waste pieces were not wanting, and were certainly intended to be melted up, there can be no doubt that we have here the indications of an establishment in which articles were cast and repaired. It was probably in time of danger, perhaps of a hostile attack, that the possessor of these numerous articles packed them so closely into the smallest possible space, without being able to recover his property, as, fortunately for us, its recent discovery proves. No doubt from such a workshop, of which, perhaps, we have by no means all the remains before us, exportation was carried on, as this would be easier than the export of pottery, even over the mountains.

Thus it would seem that we have here a source of bronze objects for Italy and perhaps also for Switzerland, but at the same time, it must be remem-

bered that the articles found show differences from those of the Swiss Lake-stations. Many objects, such as the large sickles and lance-heads, the most abundant form of fibulæ found at Bologna, the protections for the arm made of bronze wire, the elegant hollow cylinders, probably ornaments for the hair (*tutuli*), axes such as would serve for chopping wood, and large nails from six to eight inches long, are either rare or entirely wanting so far as we know north of the Alps, or, in place of them, other forms are found. On the other hand we miss, at Bologna, the large, hollow, engraved bracelets, and the round-headed hairpins of Switzerland.

The determination of the age of these remains would be important, as it must at the same time throw light on that of the Swiss lake-stations. Whether belonging to the first development of the Etruscans ("Proto-Etruscans") or to another people, the civilization of Villanova and Bologna was certainly earlier than the true great Etruscan period. Upon this question, and in what relation if any these inhabitants stood to the Mediterranean peoples who in the fourteenth century before our era made head against the Pharaohs both by land and sea, further information is much to be wished for. —(*Neues Jahrb. für Min. Geol. &c.*, 1878, Heft 3.)

ASTRONOMY.

Transit of Venus in 1874.—It would seem we may now expect a succession of new determinations of the sun's distance from observations made on this transit; for the Continental and American astronomers have still to announce their results, and from Greenwich alone we have already received several different determinations. It will be remembered that the first result announced from Greenwich made the solar parallax 8·76″, corresponding to a mean solar distance of 93,375,000 miles. The Astronomer Royal expressed the opinion that this value could not be changed by any re-examination of the observations, and that considered in its relation to other values, it should be preferred to all. "I think there will be nothing to compete with the value we have deduced," he said. We ventured at the time to express a very different opinion, namely, that this value would have to be modified largely, and that it could not compete with the estimate of about 92,300,000 deduced by Newcomb. The result has confirmed our views. All the observations have been re-examined; "different interpretations have, in a few instances, been put on the records; several observations from colonial stations have been combined; instead of using different phases in the observations (both of ingress and of egress) attempts have been made to ascertain the one phase of "contact of limbs;" the notes of a few unpractised observers have been rejected; and the result for parallax has been increased to 8·82″ or 8·83″, corresponding to a mean solar distance of 92,676,000 or 92,571,000 miles, or say to a mean distance of 92,620,000 miles. This is a long stride in the direction we predicted.

Photography in the Transit Observations.—The results from photography have been very disappointing. "The failure," says Sir George Airy, in the Greenwich report, "has arisen, perhaps, from irregularity of limb, or from atmospheric distortion, but more frequently from faintness and want of clear

definition. Many photographs, which to the eye appeared good, lost all strength and sharpness when placed under the measuring microscope. It was once remarked to me, 'you might as well try to measure the Zodiacal light.' A final result, 8·17″, was obtained from Mr. Burton's measures, and 8·08″ from Captain Tupman's." These values correspond to mean solar distances of 100,050,000 miles and 101,164,000 miles, which are of course utterly untenable.

The Transit of Mercury.—The weather on May 6 was not favourable in this country or in France. The contacts were observed at Göttingen, Josephstadt, Kiel, Antwerp, San Fernando, and Christiania. At Kiel and San Fernando a difference of time was noted between apparent and real contact; elsewhere none seems to have been noted. In the United States many important observations were made, both by American astronomers and the French Expedition. We learn that at the observatory of Dr. Henry Draper, Hastings, on the Hudson, a number of observers availed themselves of the admirable instrumental resources provided by Dr. Draper, and the weather being favourable very good results were obtained; several of the negatives taken by Dr. Draper were particularly good. Observations were made also at Washington by Professor Newcomb; and at other established astronomical observatories. The mean time of first internal contact reduced to the earth's centre was in every case earlier than the calculated time by an interval varying from 14·1 seconds at Washington to 37·7 seconds at Göttingen.

Physical Phenomena Observed at the Transit of Mercury.—The bright spot was seen by several observers, and not seen by about as many more. If those who saw it had all seen the same spot, the positive would overweigh the negative: but this was by no means the case. Some saw the spot in the middle of Mercury's disc, some saw it eccentrically situated; to some it appeared as a well defined point, to others as a misty patch; to some single, to others double. Remembering further the utter improbability, we may almost say the physical impossibility, that there could really be present on the surface of Mercury a spot bright enough to be seen under the circumstances in which a transit is observed, we cannot hesitate to regard the spot as a merely optical phenomenon. As to the ring around Mercury the case seems almost as clear, but not quite so. On any physical interpretation of a ring either dark or bright seen round Mercury, a bright arc should be distinctly visible round the part of Mercury not yet entered on the sun before first internal contact, and round the part which has passed outside the solar disc after second internal contact. Nothing of the sort seems to have been noticed on May 6. If when all the accounts, and especially those from America, have been received, this should turn out to be the case, we must reject the observations of a ring seen round the planet when on the sun, as unquestionably illusory or optical phenomena only. That this is so, is strongly suggested by the varying nature of the phenomena seen by different observers. Some saw a bright ring, others a dark one; some a broad ring round a narrow one: the bright ring was uniform as seen by some, a mere bright border to a dusky ring, as seen by others; complete as seen by most observers, incomplete as seen by one observer. On the whole, it seems tolerably certain that the ring is an entirely subjective phenomenon, and

quite certain that it is in great part subjective. On this view of the matter, some of the observations seem more worthy than the rest of special notice. [Since the above lines were written, however, news has been received from Professor Young, of Princeton, America, which seems important. He observed that the rice-grains close to the edge of Mercury seemed, if anything, lengthened radially. This was doubtless either an illusion or an accidental relation; but as, if Mercury had an atmosphere of appreciable breadth, the rice-grains would have been elongated the other way—that is, at right angles to the direction of radii of Mercury's disc—we may safely infer that the atmosphere of Mercury is of very small depth. The bright and dusky rings must then be regarded as optical illusions.]

Early Solar Photograph Showing Granules.—Dr. Rutherford, of New York, has presented to the Astronomical Society a copy on glass from a negative of the sun, made at his observatory on August 11, 1871. It represents a small portion of the sun's disc with one spot and a group of smaller ones. He calls attention to the fact that with a proper lens the granulations, rice-grains, or willow-leaves are quite clearly visible. M. Janssen was therefore mistaken when he asserted that his photographs are the only ones, and the first, which show the granulations on the sun's surface, though Dr. Rutherford admits freely the superiority of M. Janssen's recent solar photographs.

Oxygen Lines in the Sun.—Mr. Christie has made observations upon the solar spectrum near the G line, which appear to throw some degree of doubt on the supposed evidence for the existence of oxygen in the sun obtained by Dr. H. Draper. When this region was examined with high dispersion the bright lines seemed much broader than the dark ones, and did not seem softened off either edge, as might be expected if they really were bright lines, and not parts of the continuous spectrum cut out or left by dark lines. He also found certain dark lines towards the middle of each of the bright bands or spaces. This he considers renders the hypothesis that the bright bands are really bright lines still more difficult, "as one would have to assume that the oxygen lines were each double, and were only separated by a very fine sharply defined interspace." The latter inference is not valid, however, for there is no reason why dark absorption lines should not be seen on the broad bands of the spectrum of oxygen. We know even that dark lines are usually seen on the bright bands of these elements, which increase the complexity of the spectrum by widening their lines. This is caused by the absorption of the rarer and cooler vapour of these same elements, lying outside the inferior hotter vapour. This part of the evidence is in favour of the existence of bright bands, as distinguished from mere spaces between dark lines. Whether the other point may not be similarly explained,—that is, the sharply defined edges of the broad bands accounted for by the action of absorption vapours lying above the layers of oxygen giving the bright bands,—remains yet to be seen. The idea advanced by Dr. Schuster that oxygen is not probably present in the sun in both the forms whose spectra have been obtained in terrestrial experiments, seems to have very little in its favour. On the whole it does not yet appear that the very strong evidence advanced by Dr. Draper in favour of the existence of oxygen in the sun has been materially shaken. It would, however, be rash to speak at all positively at the present stage of the inquiry.

Supposed New Spot on the Moon.—The "Cologne Gazette" announces that Herr Herman J. Klein has discovered a large lunar crater of recent formation. It is situated in the large plain lying to the west of Hyginus. It presents at the time of the first quarter the appearance of a vast cavity filled with shadow, and having a diameter of about 4,400 yards. The discovery, or rather the existence of this cavity, has been confirmed by Schmidt, of Athens, and others; whether it really is a new formation remains to be seen. Nothing is shown in this space in Mr. Mädler's large map, but in Rutherford's "Photographs of the Moon" at and near the time of the first quarter, the plain is by no means so level as it might be inferred to be from Mädler's chart.

Photography at the least refrangible end of the Solar Spectrum.—Captain Abney read a preliminary paper before the Astronomical Society two years ago, upon the possibility of photographing the red end of the spectrum. He has now made considerable progress in his researches, and has laid the results before the Astronomical Society. The first photograph was one taken through three prisms of dense flint glass, placed at the angle of minimum deviation for B. The focal lengths of the collimator and camera were 18 inches and 2 feet respectively, and a condensing lens of 6 feet focus was employed to collect the light, the middle of the collimating lens alone being filled with solar rays. In front of the slit was placed a plate of orange glass to cut off the suffused blue rays, which experience had shown to be inimical to the production of good negatives, owing to the light dispersed in the prisms themselves. The impressions of bands in the ultra red are found an enormous distance below A. Roughly speaking, A corresponds to wave-lengths of 7,600 tenth-metres, and D 5,900 tenth-metres, while the photographs show bands as far below A as would correspond to wave-lengths of 10,400 tenth-metres. But this negative, remarks Captain Abney, "has no practical scientific value, as the ultra red is so tremendously compressed that the absolute wave-lengths could not be obtained from it." Using, however, a speculum-metal grating by Rutherford, having about 8,600 lines to the inch, Captain Abney was able to photograph the spectrum of the second order. With a grating of about twice as many lines to the inch, another photograph was taken, of which the Astronomer Royal for Scotland remarks that, viewing them with high magnifying power in a compound microscope long since arranged for such things, the effect was astounding. "I almost thought," he adds, "I was back again at Lisbon, viewing the sun's spectrum itself as I used to see it." This acknowledgment of the value of the photograph was peculiarly gratifying as coming from an astronomer who had paid special attention to this part of the spectrum. After describing further operations, and the methods used, which belong rather to physical than astronomical or optical matters, Captain Abney remarked in conclusion, that he hoped in a short time to reduce the expenses considerably.

Phenomena for the Quarter.—On July 25, at 9 A.M., Jupiter will be in opposition to the sun. On July 29 there will be a total eclipse of the sun not visible at Greenwich. On August 12 the moon will be partially eclipsed. The following are the epochs of the principal phenomena:—

		Mean time at Greenwich. h m	
First contact with the penumbra,	Aug. 12	9 22·5	P.M.
„ „ „ shadow		10 42·6	
Middle of the eclipse . . .	Aug. 13	0 8·2	A.M.
Last contact with the shadow		1 33·8	
„ „ „ penumbra		2 53·9	
Magnitude of the eclipse (Moon's diameter = 1) .		0·590	

First contact 109° from N. pt. of limb towards E.
Last „ 157° „ „ towards W. } For direct image.

BOTANY.

The Grasses of Mexico.—M. Eugène Fournier has studied the Mexican grasses to such good purpose that he has raised the number of species to 638; whilst in the "Flora Mexicana" of Kunth's *Synopsis* only 88 grasses were recorded. He finds that 16 species grow under very varied conditions of life, extending from the littoral regions to the elevated plateaux. He also finds, as he had previously done in regard to the ferns, that many species grow on both slopes of the Andes. He gives the following table showing the number of species peculiar to Mexico and common to it and other regions:—

Peculiar to Mexico	376
Common to Mexico and Texas	32
„ „ „ the United States	60
„ „ „ the Antilles	98
„ „ „ the Tropical region (north of Brazil and incl. Trinidad) .	102
„ „ „ the Andes	28
„ „ „ Brazil	98
„ „ „ the Argentine Republic . .	22
„ „ „ the Old World	30

The peculiar species are very numerous and include nearly all the Mexican species of the genera *Stipa*, *Aristida*, *Mecklenbergia*, *Pereilema*, *Lycurus*, *Epicampes*, *Deyeuxia*, *Trisetum*, and *Bromus*. They are related generically to the species of the Andes of South America. Fifteen genera, out of 173, belong exclusively to Mexico and eleven of these include only a single species.

Out of 262 species common to Mexico and other regions only two grow in California. East of the Rocky Mountains, Texas has 32 species; one grows in the prairie region, and the other 59 United-States species are almost confined to Florida, Georgia, and South Carolina, scarcely any occurring in Louisiana, Mississippi, or Alabama. The cause of this peculiar distribution may be found in the direction of certain winds, especially the whirlwinds noticed by M. F. F. Hébert, which, descending upon the Gulf of Mexico by the Valley of the Rio del Norte, strike the North of Florida and then sweep northwards along the eastern slope of the Alleghanies.

By their geographical distribution and botanical characters the Mexican

Gramineæ are divided very clearly into two groups—those which are peculiar to Mexico or common to it and the Andean region, or to more northern regions, are generally distinguished by the slenderness and lightness of their leaves and panicles; those which spread into the tropical region, on the contrary, are remarkable by their size and the amplitude of their organs of vegetation and of their inflorescence. The former generally inhabit dry and mountainous localities; the latter the banks of rivers, and moist places. Some of them extend from the United States to the Argentine Republic, through 70° of latitude.—(*Comptes rendus*, June 10, 1878.)

The Influence of Moisture on Vegetation.—Dr. Paul Sorauer has made some experiments with spring barley which led to the following results:—In dry air branching was greater than in moist, the mean figures standing at 2·77 and 2·37 respectively; the length of leaves was greater in moist air in the ratio of 21·37 to 21·07, but the breadth was less (6·74 to 7·33). A moist atmosphere is more favourable to length of leaf-sheath in the proportion of 9·26 to 8·18, to the growth of the principal stem as 13·5 to 11·5, and to root-development as 26·8 to 23·9. The epidermal cells of the leaves were more numerous and broader, the cells between the stomata shorter, and the stomata themselves shorter in dry air. Leaves developing in a moist atmosphere have fewer stomata per millimetre of length.—(*Bot. Zeit.* January, 1878, and *Journ. of Botany*, March 1878.)

The Fungi of the Vine.—The study of the fungi infesting the vine is one of considerable interest, not only to the scientific botanist, but also in a high degree to all interested in the cultivation of the vine. Baron G. Felix von Thümen has published an elaborate monograph upon these parasites, containing an enumeration of no less than 220 species of fungi which occur upon vines in various parts of the world. The fungi are distributed as follows upon nine species of the genus *Vitis*:—*V. vinifera*, Lin., 150; *V. Labrusca*, Lin., 54; *V. æstivalis*, Mich., 13; *V. vulpina*, Lin., 7; *V. riparia*, Mich., 3; *V. cordifolia*, Mich., 3; *V. rotundifolia*, Mich., 3; *V. caudicans*, Engelm., 1; and *V. sylvestris*, Gmel., 2. The fungi affect the leaves, fruit, stems, branches, and roots of the plants. Nineteen fungi affect the grape itself, and of these *Oïdium Tuckeri* is the most important from an economical point of view. Baron von Thümen appears to follow Fuckel in regarding it as the conidial form of *Sphærothera Castagnei*, Lév., and therefore distinct from the American *Erysiphe nectator*, Schw., which species does not seem to have been recorded since Schweinitz's time; while it is classed by Dr. Cooke, in "The *Erysiphei* of the United States" among the "species dubiæ." Baron von Thümen also considers it unconnected with a variety of *Uncinula spiralis*, B. and C., which sometimes occurs upon the grape in the United States.

Among the great number of fungi infesting the living and dead stems and branches of the vines there are many new species. Of the Hymenomycetous fungi, twenty-two species are mentioned, mostly occurring on the dead stems; these include a small agaric from the Cape of Good Hope (*Agaricus proteus*, Kalchbr.) allied to *Ag. variabilis*, P. One of the most interesting of the fungi on living vine-leaves is a new species of *Uredo* (*U. Vitis*, Thüm.), which, like *Peronospora vitis*, is of transatlantic origin.

The difference between the American and European vine-flora is very

striking, so many fungi occurring in America which have not been observed in Europe, where vine-cultivation is such an important industry, and mycologists are so much more numerous. Many of the old species of Schweinitz have never been met with here.

Cincinnobolus Cesatii, De Bary, is a curious mould which grows parasitically upon *Oïdium Tuckeri*; it appears to be common on the Continent, but has not yet been observed in this country.—(*Journ. of Botany*, May 1878.)

The Connection of Bacilli with Splenic Fever.—Dr. Cossen Ewart publishes in the "Journal of Microscopical Science," an account of some investigations which he has made upon this subject, doubt having of late been thrown upon the supposed fact that splenic fever is caused by *Bacillus anthracis*. In the spleen of a mouse which had just died of the disease after being inoculated with the plant, the *Bacilli* were found rod-shaped and motionless. In a few hours, the temperature being 30° C. (=91° F.), many of these rods began to move actively in a wriggling manner, and after continuing in this motile state for some time, they either suddenly or gradually settled down again; and then lengthened out into spore-bearing filaments. A mouse inoculated with the spores thus obtained, died in 48 hours of splenic fever. The spores are formed in a similar manner to the Chlamydospores of *Mucor*. By Koch and some other observers these spores have been described as germinating at once, and reproducing the rods, but Dr. Ewart found that this process was often preceded by the division of the spore into four sporules, all of which closely adhere at first, but ultimately become free and settle down in colonies. The sporules then germinate and produce the rods. The most important morphological conclusion which Dr. Ewart draws from these and other observations is that the *Micrococcus-*, *Bacterium-* and *Bacillus*-forms, and the spore-bearing hyphæ are phases of the same life-history. His experiments have been confirmed by other observers. It would be unwise to push the suggested analogy between *Bacillus* and *Mucor* too far, since the true *Mucor* is a member of the most highly organized group of fungi and *Bacillus* undoubtedly belongs to the lowest.—(*Academy*, June 22, 1878.)

Studies on the Mucorineæ.—M. Van Tieghem's third memoir on these plants ("Ann. des Sci. Nat. Bot.," sér. vi. tome iv.) contains valuable observations on some points of interest in connexion with the group. He first deals with the mutilation and fragmentation of reproductive cells. Zygospores of *Sporodinia grandis* and *Spinellus fusiger* were chosen for experiment on the one hand, and asexual spores of *Pilobolus Œdipus*, *Phycomyces nitens* and *Mortierella reticulata* on the other. It was found to be a necessary condition that the body to be mutilated should be proximately homogeneous; so that in the case of zygospores the experimentation must occur either before perfect maturity, or after commencement of germination when the oil has disappeared. A second condition is that the fragment must not be too small. The process adopted was as follows:—Buddings from a zygospore germinating in a humid atmosphere were cut off as they appeared; after a time the contents of the Zygospore divided into a number of spores, separated by interstitial matter; the same result was obtained by similar treatment of an asexual spore; and if schizomycetous organisms were admitted into the culture-fluid, these endospores were produced without any scission of buds.

M. Van Tieghem considers the cause which favours the formation of zygospores to consist in the air being impoverished of oxygen. He describes the germination of spores and zygospores in different media, and the structure and mode of dehiscence of the sporange in *Pilobolus* and *Pilaira*.

In the systematic portion of his memoir he describes new species of *Pilobolus, Rhizopus, Helicostylum, Thamnidium, Mortierella*, and *Syncephalis*, and characterizes a new genus, *Absidia*, with four species. This genus may be said to have the asexual growth of *Rhizopus*, and the sexual growth of *Phycomyces*. From the former it differs chiefly in having the sporangiferous stolons of a parabolic form and cuticularized throughout, the sporangia alternate with the rootlets, the columella conical and slender, the membrane of the spores not cuticularized and coloured, and the zygospores furnished with a *Phycomyces*-like investment. If a spore is kept in a nutritive medium it produces a mycelium; in a moist atmosphere, after having attained a certain size, it gives origin directly to a sporange without forming any mycelium. The zygospore behaves in a similar manner. Asexual spores, similar to the zygospores, are sometimes produced by a parthenogenetic process; these are termed *azygospores*.

The following is M. Van Tieghem's division of the Mucorineæ into tribes:—

No stylospores. A columella in the sporange, the membrane of which is. . . .	Heterogeneous, *i.e.*, formed of an upper cuticularized hood and a lower diffluent zone	*Piloboleæ.*
	Homogeneous, either all entirely persistent, or all diffluent . . .	*Mucoreæ.*
Stylospores present. No columella in the sporange, which is .	Spherical and isolated	*Mortierelleæ.*
	Cylindrical and grouped in capitula	*Syncephalideæ.*

—(*Journ. of Botany*, May 1878.)

Non-sexual Outgrowths on the Prothalli of Ferns.—The discovery of the non-sexual production of ferns from the prothallus originally made by Dr. Farlow has been confirmed by observations made upon several species. At the meeting of the Society of German Naturalists, held last year at Munich, Professor De Bary reported upon the subject as follows:—"Investigation has shown that some ferns, namely, *Pteris cretica, Aspidium falcatum*, and *Aspidium filix-mas*, var. *cristatum*, form normal antheridia on the prothalli, but generally no archegonia, or only imperfect ones which perish before the opening of the canal, and exhibit, instead of the typical formation of the embryo, the outgrowth described by Farlow. In those species which develope archegonia, no such outgrowth has been observed. The morphological phenomena presented by the shoot are alike in the species named, even in the smallest particulars. In the prothallus, which grows directly out of the spore, the development which occurs in most cases, and may be called normal, is as follows:—At the sinus, nearly at the spot where the first archegonium occurs, there arises a protuberance which grows out directly into a leaf; at the base of this, close by the insertion into the prothallus, there is formed an aerial *punctum vegetationis*, on which a second and the successive later leaves appear. At the base of the first leaf there is formed, endogenously on the vascular bundle, the first root. As soon as the second leaf appears the bud grows like an ordinary fern-shoot. Varia-

tions from the normal mode of growth are not uncommon. Frequently the prothalli form branches like themselves (secondary prothalli) which can again produce leaves and shoots in a great variety of forms. A moment's reflection shows that the three ferns in question have lost the power of forming archegonia, and with it, sexual reproduction; and as compensation for the loss of the sexual formation of the embryo, possess the power of forming the outgrowth described. This presents a special case of that general phenomenon which is called *apogamy*, total loss of reproductive power, and which consists in the fact that a species loses sexual reproduction, and acquires in its place numerous non-sexual modes of propagation, such as brood-buds, suckers, &c. The numerous bulblets of the higher Phanerogams, species of *Allium*, *Deutaria*, and the like, are examples of this, as well as of the successive degrees of difference in apogamy."—(*Silliman's Journal*, May 1878.)

CHEMISTRY.

Two allotropic Modifications of Magnetic Oxide of Iron.—When iron sesquioxide is heated for some hours to 350°—440° C. in an atmosphere of hydrogen or carbonic oxide, it is converted into the magnetic oxide. This substance is identical in properties with the product obtained either by calcining the hydrate of the magnetic oxide at 300° in a gas which is without action upon it, or by the decomposition of the carbonate of iron, or by the ignition of pyrophoric iron protoxide at a low red heat. It differs in a marked degree from that form of the magnetic oxide which we obtain on the employment of higher temperatures, whether by the decomposition of water by red hot iron, by burning iron in oxygen, or by the splitting up of the sesquioxide at a bright red heat. Magnetic oxide of iron, Fe_3O_4, is black and is strongly attracted by the magnet; that variety, however, which is formed at lower temperatures by the processes referred to has a specific gravity of 4·86, is acted upon by concentrated nitric acid, and can undergo further oxidation, being changed by rusting into sesquioxide. The magnetic oxide formed at the higher temperatures is not changed into sesquioxide when heated in the air, is not attacked by strong nitric acid, and has a specific gravity of 5·0—5·09. This is the variety which forms the little granules and spherules found in meteoric dust. Moissan, who has made these observations ("Compt. rend." 1878, lxxxvi. 600) determined some time since the existence of two allotropic modifications of iron protoxide: a pyrophoric modification obtained at temperatures between 350° and 450°, and a second and non-pyrophoric variety produced, as Debray has shown, at temperatures between 1000° and 1200°. The first variety when oxidized is converted into sesquioxide, the second forms magnetic oxide only, and of the kind which is produced at high temperatures. If pyrophoric iron protoxide, which would be converted into sesquioxide by exposure to the air, be heated to a low red heat in a current of carbonic acid, it is oxidized to magnetic oxide, carbonic oxide being formed; the variety of the magnetic oxide obtained by this operation is identical in character with that obtained at low temperatures. If magnetic

oxide, prepared at 350°, be raised to bright red heat in a current of nitrogen it shrinks together, its specific gravity is changed, it can no longer be converted into sesquioxide; in short it possesses all the qualities of the magnetic oxide produced at 1200°. The result is the same when carbonic acid is employed in place of nitrogen, for the experiments of Gruner in 1872 established the fact that in a strongly heated atmosphere of carbonic acid, magnetic oxide alone is produced.

Revision of the Atomic Weight of Antimony.—Prof. J. P. Cooke has determined anew the atomic weight of this metal, and has devoted to that object much time and great care. He first found the weight of antimony sulphide, Sb_2S_3, obtained from a known quantity of antimony, taking the atomic weight of sulphur as 32. The red variety was dried at 180° C., and the grey modification in most instances at 210°. A mean of thirteen experiments, where the sulphide was dried at 180°, furnished the number 119·994, and a mean of eleven others where the compound was heated to 210° gave 120·295 as the number representing the atomic weight. In each case the author convinced himself that no free sulphur was present, and he took precautions to eliminate every possible source of error. In a second series of no less than seventeen estimations, where antimony chloride was employed, the atomic weight of the metal was found to be 121·94, a number which closely corresponds with that obtained by Dumas and Dexter. In this case, it should be stated, the atomic weight of chlorine was taken as 35·5, and that of silver to be 108. As each of the two series of experiments made by Cooke furnished numbers which closely correspond *inter se*, while the final results differ considerably, the author set himself to determine the atomic weight of sulphur. By reducing silver sulphide at a low red heat with dry hydrogen he found sulphur to be 31·158 if silver is 108, and 32·137 if silver is 107·93. In a fourth series of estimations Cooke determined the amount of bromine in antimony bromide, $SbBr_3$, and arrived at the number 120 as a mean of fifteen analyses. When he employed antimony iodide he again obtained the number 120, if the atomic weight of iodine be taken to be equal to 127 and that of silver 108; or 119·95 when the atomic weights of those elements are respectively held to be 126·85 and 107·93. The correct number appears to be 120, and the disparity appears to have arisen from the fact that it is impossible to prepare antimony chloride absolutely free from oxychloride ("Amer. Journ. Sc." 1878, xi. 41.)

A new Product of the Oxidation of Lead.—Red lead, as is well known, is prepared by heating litharge (lead oxide) in a fine state of division to 500° in presence of air. The oxide slowly takes up 2·3 per cent. of its weight of oxygen, and is converted into an oxide represented by the formula Pb_3O_4. Dumas regarded this body as a compound of lead oxide and lead peroxide—

$$2PbC + PbO_2 = Pb_3O_4.$$

Berzelius, on the other hand, believed it to consist of lead oxide and lead sesquioxide, and to resemble magnetic oxide of iron (Fe_3O_4) in point of constitution. Although he did not succeed in isolating the sesquioxide, it has now been shown by Debray ("Compt. rend." 1878, lxxxvi. 513) that such an oxide can under certain circumstances be formed. When lead peroxide is heated to 440° a brisk evolution of oxygen is observed to take place.

After the lapse of four or five hours the substance is entirely converted into red lead. If the lead peroxide be heated to 350° gas is plentifully evolved, but the action very soon ceases, and a product is obtained having exactly the composition of lead sesquioxide. It is a greenish-brown powder, which both in properties and in point of composition stands intermediate between red lead and lead peroxide. It can also be produced by heating lead oxide or, what is better, lead carbonate to 350° in a current of air or oxygen. The cause of the failure of the earlier chemists to isolate this oxide is clearly to be traced to their having employed too high temperatures.

The Restoration of Faded Handwriting.—In a paper on several interesting points connected with chemical technology ("Journ. prakt. Chem.," 1878, xvii. 83) von Bibra discusses the best means of rendering legible writing which in process of time has become obscure. He refers to the custom of employing freshly prepared ammonium hydrosulphate, which, he states, is to be applied with a brush, the excess to be removed with water and the paper or parchment then quickly dried between folds of blotting-paper. In the case where an important document is to be deciphered we cannot help regarding this as little else than heroic treatment, and prefer to pour the reagent in a watch-glass and to hold the manuscript over it, exposing it to the fumes of the strong hydrosulphate just so long as is necessary to develop the handwriting. At best the method is not a good one, because it converts the iron of the ink into the metallic sulphide which rapidly undergoes oxidation; the writing in a very short time will fade away as the sulphate is diffused through the moist paper, and the characters will appear blurred when any subsequent attempt is made to revive them. Von Bibra has now found in a moderately concentrated aqueous solution of tannin (gallotannic acid) an agent which produces the desired result, and at the same time possesses none of the disadvantages of the hydrosulphate. He applies the tannin solution with a brush, removes the excess by a current of water, and dries the document at a temperature of 50°—60° R. The writing developed in this manner is clear and very black, remaining so after the lapse of several months. These methods, it need hardly be stated, refer only to manuscripts where ordinary ink has been employed, and not to documents written with Indian or carbon ink.

The Chemical Action of the Electric Discharge on Persulphuric Acid.—As this acid can be formed by the action of oxygen on sulphurous acid (*see Popular Science Review*, April, 1878, 207) it occurred to Berthelot that it may also be prepared by the direct action of oxygen on sulphuric acid $S_2O_6 + O = S_2O_7$. This fact he has now established ("Compt. rend." 1878, lxxxvi. 277). He subjected 277 milligrammes of sulphuric anhydride in contact with 60 ccm. of dry oxygen to the action of the electric discharge for the space of eight hours; the tube containing these materials was then heated to remove a few little drops of unaltered anhydride, and he then continued the transmission of the current for some hours. By this time the anhydride had completely disappeared, and in the place of it there was a substance closely resembling persulphuric acid. Rather more than 20 ccm. of the gas had disappeared. The new body possessed the composition of persulphuric acid. It was found that the anhydride and oxygen, when placed together, do not enter into combination unless the electric currents

are employed. Persulphuric acid prepared in this manner is not a permanently stable body. The beautiful crystalline needles begin to crumble away in a few days, and are soon converted into a snow-like mass, which appears to be a compound of sulphuric acid and persulphuric acid. The same substance is formed when the discharge is first transmitted, and appears consequently to be an intermediate product.

Fluoranthene.—This is a new hydrocarbon obtained by Fittig and Gebhard from coal tar ("Ber. deutsch. chem. Gesell. Berlin." x. 2141). In preparing and purifying a large quantity of pyrene, a substance was discovered having the formula $C_{15}H_{10}$, fusing at 100° and crystallizing in large lustrous plates. This is the new body. When treated with the requisite reagents it yielded the picric acid compound $C_{15}H_{10} + C_6H_3(NO_2)_3O$ and the trinitro-derivative $C_{15}H_7(NO_2)_3$. When oxidized with chromic acid solution it, like phenanthrene, evolves carbonic acid and is converted into a mixture of a chinone and an acid having the formula $C_{14}H_8O_3$; the barium salt of this body crystallizes in little warty concretions and the calcium salt in small golden-yellow needles. It is isomeric with oxyanthrachinone, but has the same empirical formula only as that body. It is undoubtedly an acid, containing beyond all question the group COOH. When distilled with finely divided zinc it yields nearly the theoretical amount of fluorene, and when heated with lime splits up into carbonic acid and diphenylenketone, which has led the authors to give it the name of diphenylenketon carbonic acid. Fittig believes the idryl of Goldschmiedt to be identical with fluoranthene.

The Reaction of Nitrogen and Water.—Berthelot, having had occasion to repeat the experiments on the formation of ammonium nitrite by means of the electric discharge, now finds that absolutely pure nitrogen when exposed in the concentric tubes, in contact with water, to the discharge of a powerful Ruhmkorff apparatus for from eight to ten hours yields ammonium nitrite beyond all question. The salt does not appear to be formed unless the discharge is a powerful one. With currents of low tension nitric acid is not formed in moist air. The formation of nitrates and nitrites in atmospheric air is assumed therefore to be due to the action of lightning ("Ann. Chem. Phys." [5], xii. 445). It must not, however, be forgotten that Zöller and Grete have reopened the inquiry whether ammonium nitrite is a product of the combustion of hydrogen in air, and the result of the experiments, conducted under most rigorous conditions, goes to show that during the combustion of absolutely pure hydrogen in perfectly pure air, small but distinct quantities of the nitrite are formed. ("Ber. deut. chem. Gesell. Berlin." x. 2144.)

GEOLOGY AND PALÆONTOLOGY.

The Date of the Last Glacial Epoch.—Mr. N. H. Winchell has communicated a paper to the Geological Society in which he endeavours to determine the date of the final cessation of glacial conditions in North America by determining the rate of recession of the Falls of St. Anthony, which are situated near the junction of the Minnesota and the Mississippi. These

falls have been known to Europeans about 200 years. The country is covered with deposits of glacial origin, and between Fort Snelling and the present Falls, a distance of eight miles, the existing river-gorge has been formed since the deposition of the newer Boulder-clay. The old river-valley is filled up with glacial deposits. The gorge is of very uniform character, being cut through hard limestones resting on soft sand rock, both lying quite horizontally. The country was settled in 1856, and the recession of the falls has since been very rapid, its rate having been accelerated by the erection of saw-mills, dams, &c. From the accounts of various travellers who have visited the falls in the last 200 years, Mr. Winchell endeavours to obtain an estimate of the true rate of recession. Between the visit of Father Hennepin in 1688 and that of Carver in 1766 he finds a recession at the rate of 3·49 feet annually; between Carver's visit and 1856, a mean annual recession of 6·73 feet; and between Hennepin and 1856, one of 5·15 feet. The time-estimates for the cutting of the gorge would be, according to the above means, 12, 103, 6, 276 and 8, 202 years. Mr. Winchell considers the data upon which the second of these numbers is founded the most reliable. Sir Charles Lyell's estimate of the recession of the Falls of Niagara from the escarpment at Queenstown to their present site is 35,000 years.

The Mosasaurs.—Professor O. C. Marsh has obtained the materials for a complete reconstruction of the fore-limb of *Lestosaurus*, an American type of Mosasaurs. Professor Owen has instituted a comparison of the characters of this limb, as shown in Professor Marsh's figure, with the bones of the same parts in cetaceans, plesiosaurs, and lizards. He finds the characters presented to approach most closely to those of the last-named order of Reptiles, of which *Monitor niloticus* was taken for comparison. Hence he regarded his view, originally stated in 1851, that the Mosasaurs form a tribe, *Natantia*, in the Reptilian order Lacertilia, as confirmed; and he compared the relations of these reptiles to the other lizards with those of the seals to the ordinary carnivorous mammals.—(*Proc. Geol. Soc.*, 5 June 1878).

Palæozoic Rocks under London.—Professor Prestwich has given a most interesting account of the results of the deep well-boring recently made at Messrs. Meux's brewery, at the south end of Tottenham Court Road. He remarks that the well-known boring at Kentish Town, in 1856, showed the absence at that point of Lower Greensand, the Gault being immediately succeeded by hard red and variegated sandstones and clays, the age of which was at first doubtful, but which were finally considered to approach most nearly to the Old Red Sandstone near Frome, and to the Devonian sandstones and marls near Mons, in Belgium. The existence of some doubt as to this identification rendered the boring lately made at Messrs. Meux's brewery particularly interesting, and the method of working adopted by the Diamond-boring Company, by bringing up sharply cut cores from known depths, gave special certainty to the results obtained. The boring passed through 652½ feet of Chalk, 28 feet of Upper Greensand, and 160 feet of Gault, at the base of which was a seam, three or four feet thick, of phosphatic nodules and quartzite pebbles. Beneath this was a sandy calcareous stratum of a light ash-colour, passing into a pale or white limestone, and this into a rock of oolitic aspect. Casts and impressions of shells found in this bed showed

it to be the Lower Greensand, whose place it occupied. The boring was carried further in the hope of reaching the loose water-bearing sands of this formation, but the rock became very argillaceous, and, when 62 feet of it had been passed through, the boring entered into mottled red, purple, and greenish shales, dipping at 35° in an unascertained direction. These beds continued through a depth of 80 feet, when, their nature being clearly ascertained, the boring was stopped. The fossils of these coloured beds, which included *Spirifera disjuncta*, *Rhynchonella cuboides*, and species of *Edmondia*, *Chonetes*, and *Orthis*, show them to be of Devonian age. Thus, the existence of Palæozoic rocks at an accessible depth under London, and the absence of the Jurassic series, as maintained long since by Mr. Godwin-Austen, is experimentally demonstrated.

These facts are of interest in connection with the question of the possible extension of the Coal-measures under the Cretaceous and Tertiary strata of the south-east of England. The beds found at the bottom of Messrs. Meux's boring are of the same character as the Devonian strata which everywhere accompany the Coal-measures in Belgium and the north of France, being brought into juxta-position with them by great faults and flexures. A remarkable section at Auchy-au-Bois, in the western extremity of the Valenciennes coal-field, is particularly interesting from its furnishing evidence that the Hardinghen coal-field, between Calais and Boulogne, is a prolongation of that of Valenciennes, and because the same strike and a prolongation of the same great fault observed at Auchy-au-Bois through Hardinghen would carry the southern boundary of any coal-field in the south-east of England just south of Maidstone, thence passing a little north of London. Hence it is in the district north of London that there is most probability of the discovery of the Carboniferous strata. The extent of country in which shafts could be sunk to the Palæozoic strata will, however, be limited by the presence of the water-bearing Lower Greensand, which probably reaches close to London in the south, reappears in Buckinghamshire and Bedfordshire, 30 or 40 miles north of London, and probably extends some distance towards the city under the Chalk hills of those counties and Hertfordshire.

The nature of the representative of the Lower Greensand in the boring, and the characters of the fossils contained in it, lead to the conclusion that in it we have a deposit produced near the shore of the Neocomian sea, here probably consisting of cliffs of Devonian (or Carboniferous) rock. From these cliffs the calcareous material which here replaces the usual loose sands of the Lower Greensand was perhaps derived by the agency of springs; and the shore-line itself must be situated between the south end of Tottenham Court Road and the Kentish Town boring. The sandy beds of the Lower Greensand will probably be found to set in at no great distance to the southward, presenting the conditions necessary for storing and transmitting underground waters. A test boring made by Mr. H. Bingham Mildmay at Shoreham Place, about five miles from Sevenoaks, and in which the Lower Greensand was met with at about the estimated depth (450 feet) and furnished a supply of water, seems to confirm these views.

A Fossil Passerine Bird from Colorado.—The wonderful insect-bearing deposits of Florissant, Colorado, have furnished the remains of a Passerine bird, the first of its type that has yet occurred on the American continent.

These remains consist of the greater part of the skeleton, but the thighbones, the beak, and the anterior part of the head are wanting. The slab containing the specimen also shows remarkably distinct impressions of the feathers of the wings and tail. In size and general proportions the bird resembles the Scarlet Tanager (*Pyranga rubra*), and the Cedar Bird (*Ampelis cedrorum*); in the development of the wings it also agrees with those birds, but the legs and feet are smaller and weaker. The total length of the bird was about seven inches. Dr. Allen regards its organization as indicating arboreal habits and considerable power of flight. The absence of the bill renders it impossible to decide as to the particular family to which this bird should be allied, but the balance of the evidence from these remains is in favour of its being a finch. Dr. Allen proposes to found a new genus for the bird, which he names *Palæospiza bella*. Other remains of birds have been found in the Florissant beds.—(*Silliman's Journal*, May 1878.)

American Permian Reptiles.—Professor Marsh notices some reptiles from a peculiar lacustrine formation in the Western Territories, but especially from New Mexico, the geological position of which appears to him the upper part of the Permian. Among them are several genera showing the more important characters of the Rhynchocephala, of which the New Zealand *Hatteria* is the sole living type. One remarkable characteristic of all the reptiles here noticed is the presence of intercentral ossifications below the biconcave bodies of the vertebræ; and another the existence of three facets for the articulation of the ribs in the anterior rib-bearing vertebræ. In the implantation of the teeth and their successional development, these reptiles resembled the Mosasaurs. They represent two families, which Professor Marsh calls Nothodontidæ and Sphenacodontidæ. The former includes a single species, *Nothodon lentus*, which was probably five or six feet long, slow in its movements, partially aquatic and herbivorous. Three species of the second family are described, namely:—*Sphenacodon ferox*, a carnivorous reptile about six feet long, with the posterior teeth compressed, with very sharp smooth edges; *Ophiacodon mirus*, also a carnivorous form, about the same size as the preceding, with conical teeth, of which the anterior are recurved as in serpents; and *O. grandis*, a larger species, about ten feet in length.—(*Silliman's Journal*, May 1878.)

The First American Jurassic Mammal.—Professor Marsh says, "One of the most interesting discoveries made in the Rocky Mountain region is the right lower jaw of a small mammal recently received at the Yale College Museum. The specimen was found in the *Atlantosaurus*-beds of the Upper Jurassic, and the associated fossils are mainly Dinosaurs.

Dryolestes priscus, gen. et sp. nov.

"This specimen is in fair preservation, although most of the teeth have been broken off in removing it from the rock. The penultimate molar, however, remains. The shape of the jaw, and the position and character of the teeth, show that the animal was a small marsupial, allied to the existing Opossums (*Didelphidæ*). The tooth preserved has the same general form as the corresponding molar of *Chironectes variegatus* Illiger. The angle of the jaw is imperfect, but there are indications that it was inflected.

"The principal dimensions of this specimen are as follows:—

Space occupied by seven posterior teeth . . .	12·5mm.
Depth of jaw below last molar	4·4
Transverse diameter	1·8
Height of crown of penultimate molar . . .	2·
Transverse diameter	1·5

"The present specimen indicates an animal about as large as a weasel. It is of special interest, as hitherto no Jurassic mammals have been found in this country."—(*Silliman's Journal*, June 1878.)

Arctic Geology.—The last British Polar Expedition under Sir George Nares hardly satisfied the expectations of the public, but we are gradually learning that its scientific results are by no means to be despised. Captain Feilden, the naturalist to the expedition, devoted a good deal of his attention to the geology of the region explored, and the fruits of his researches add considerably to our knowledge of Arctic Geology. A long paper on this subject was read before the Geological Society on April 17, by Captain Feilden and Mr. De Rance, in which they described the Laurentian gneiss that occupies so large a tract in Canada as extending into the Polar area, and alike underlying the older Palæozoic rocks of the Parry Archipelago, the Cretaceous and Tertiary plant-bearing beds of Disco Island, and the Oolites and Lias of East Greenland and Spitzbergen. Newer than the Laurentian, but older than the fossiliferous rocks of Upper Silurian age, are the Cape-Rawson beds, forming the coast line between Scoresby Bay and Cape Cresswell, in lat. 82° 40′; these strata are unfossiliferous slates and grits dripping at very high angles.

From the fact that Sir John Richardson found these ancient rocks in the Hudson's Bay territory to be directly overlain by limestones containing corals of the Upper Silurian Niagara and Onondaga groups, Sir Roderick Murchison inferred that the Polar area was dry land during the whole of the interval of time occupied by the deposition of strata elsewhere between the Laurentian and the Upper Silurian; and the examination by Mr. Salter, Dr. Haughton, and others of the specimens brought from the Parry Islands have hitherto been considered to support this view. The specimens of rocks and fossils, more than 2,000 in number, brought by the late expedition from Grinnell and Hall Lands have made known to us, with absolute certainty, the occurrence of Lower Silurian species in rocks underlying the Upper Silurian; and as several of these Lower Silurian forms have been noted from the Arctic Archipelago, there can be little doubt that the Lower Silurians are there present also. The extensive areas of dolomite of a creamy colour abounding in fossils discovered by M'Clintock around the magnetic pole, on the western side of Boothia, in King William's Island, and in Prince of Wales Land, described by Dr. Haughton, probably represent the whole of the Silurian era and possibly a portion of the Devonian.

The bases of the Silurians are seen in North Somerset, and consist of finely stratified red sandstone and slate, resting directly on the Laurentian gneiss, resembling that found at Cape Bunny and in the cliffs between Whale and Wolstenholme Sounds. Above these sandstones occur ferruginous limestones, with quartz grains, and still higher in the series the

cream-coloured limestones come in. The Silurians occupy Prince Albert Land, the central and western portion of North Devon, and the whole of Cornwallis Island. The Carboniferous Limestone was discovered, rising to a height of 2,000 feet, on the extreme north coast of Grinnell Land, in Feilden and Parry Peninsulas, and contains many species of fossils in common with the rocks of the same age in Spitzbergen and the Parry Archipelago, being probably continuously connected with the limestone of that area, by way of the United States range of mountains. The coal-bearing beds that underlie the Carboniferous Limestones of Melville Island are absent in Grinnell Land, but they are represented by true marine Devonians, established in the Polar area for the first time, through the determination of the fossils, by Mr. Etheridge. In America a vast area is covered by Cretaceous rocks. The lowest division, the Dakota group, contains lignite seams and numerous plant-remains indicating a temperate flora; overlying the Cretaceous series are various Tertiary beds, each characterized by a special flora, the oldest containing subtropical and tropical forms, such as various palms of Eocene type. In the overlying Miocene beds the character of the plants indicates a more temperate climate, and many of the species occur in the Miocene beds of Disco Island, in West Greenland, and a few of them in beds associated with the 30-feet coal-seam discovered at Lady Franklin Sound by the late expedition. The warmer Eocene flora is entirely absent in the Arctic area, but the Dakota beds are represented by the 'Atane strata' of West Greenland, in which the leaves of dicotyledonous plants first appear. Beneath it, in Greenland, is an older series of Cretaceous plant-bearing beds, indicating a somewhat warmer climate, resembling that experienced in Egypt and the Canary Islands at the present time. In the later Miocene beds of Greenland, Spitzbergen, and the newly-discovered beds of Lady Franklin Sound, the plants belong to climatal conditions 30° warmer than at present, the most northern localities marking the coldest conditions. The common fir (*Pinus abies*) was discovered in the Grinnell Land Miocene, as well as the birch, poplar, and other trees, which doubtless extended across the Polar area to Spitzbergen, where they also occur.

At the present time the coasts of Grinnell Land and Greenland are steadily rising from the sea, beds of glacio-marine origin, with shells of the same species as are now living in Kennedy Channel, extending up the hill-sides and valley slopes to a height of 1,000 feet, and reaching a thickness of from 200 to 300 feet. These deposits, which have much in common with the 'boulder-clays' of English geologists, are formed by the deposition of mud and sand carried down by summer torrents and discharged into fiords and arms of the sea, covered with stone- and gravel-laden floes, which, melted by the heated and turbid waters, precipitate their freight on the mud below. As the land steadily rises these mud-beds are elevated above the sea. The coast is fringed with the ice-foot, forming a flat terrace 50 to 100 yards in breadth, stretching from the base of the cliffs to the sea-margin. This wall of ice is not made up of frozen sea-water, but of the accumulated autumn snowfall, which, drifting to the beach, is converted into ice where it meets the sea-water which splashes over it. As a supplement to this memoir a very extensive and important paper on the fossils brought by Captain Feilden, was read by Mr. Etheridge, in which he remarked espe-

cially on the presence of undoubted Silurian and Carboniferous fossils at the highest latitude yet reached, namely 82° 45′ north.

Stromatopora and Loftusia.—Principal Dawson has contributed a statement of his views as to the nature of those problematical fossils from the Palæozoic rocks which are commonly known as *Stromatopora*. They are massive fossils, often showing concentric structures when weathered, and have been referred by different writers to the Sponges, to the Corals, to the Hydroida as allied to *Hydractinia*, and to the Foraminifera. Principal Dawson says that *Stromatopora* is "a calcareous, non-spicular body, composed of continuous concentric porous laminæ, thickened with supplemental deposit, and connected by vertical pillars, most of which are solid;" and he maintains his old opinion that *Stromatopora* is a Foraminiferal organism and the Palæozoic representative of the Laurentian *Eozoon*.

Dr. G. M. Dawson has made the interesting discovery of a species of *Loftusia* in a Limestone, probably of Carboniferous age, which occurs in British Columbia in the banks of Marble Cañon, Frazer River. He names this species *Loftusia columbiana*. As the genus *Loftusia* was established for the reception of a tertiary Persian fossil Foraminifer, this discovery is of great interest provided the reference of the North American form to this genus be confirmed. *Loftusia columbiana* is much smaller than *L. persica* (0·3 inch long); it occurs here and there in patches in the limestone, but the number of specimens when it does appear is very great.—(*Proc. Geol. Soc.*, June 5, 1878.)

MICROSCOPY.

The New Oil-immersion Object-glass.—The Rev. W. H. Dallinger writes to "Nature":—"As a piece of workmanship this lens is extremely fine; and it can be used with quite as much ease as an ordinary immersion quarter-inch objective. It works admirably with Powell and Lealand's ordinary sub-stage condenser, with Wenham's reflex illuminator, and with the small plano-convex lens which the maker sends with it to be fastened to the under surface of the slide with the oil of cedar wood. But I have also secured admirable results with the illuminating lens of Powell and Lealand's supplementary stage, which gives entire command over the angle of the illuminating ray.

"The spherical aberration is beautifully corrected, the field being perfectly flat. The colour corrections are, so far the lens goes, equally perfect; but are somewhat conditioned by the dispersive power of the oil, which can be modified readily. The sharpness and brilliance of the definition which this lens yields are absolutely unsurpassed, in my experience; and it has a very great power of penetration.

"I tested it with a series of tests with which I have proved and compared the glasses of various makers in England, the Continent, and America, for some years. Up to the time of receiving this lens, the $\frac{1}{8}$-inch that had done the most in my hands was one of the 'new formula' lenses of Powell and Lealand. It is but justice to say that all my most crucial tests were equally mastered by the lens of Zeiss. I have not been able to do more with it than with the English glass, *but the same results* can be accom-

plished *much more readily*. The correction has to be brought into operation, and careful adjustment made, to get the finest result with the English lens; but the German glass has simply to be brought into focus, and the best result is before the observer, provided that the light has been adjusted in the most efficient manner. It is true that for sharp and perfect definition we must be careful to adjust the length of the draw-tube; in working this lens there is much need of attention to this matter; and speaking from a practical point of view, it takes the place, in securing crisp definition, of the screw-collar adjustment, although, of course, much easier of application. But it is so easy to work the lens with fine results on the more delicate tests, that I think that those who make the resolution of these their primary object in the possession of a microscope, can scarcely fail in securing their utmost desire. It is a glass pre-eminently suited for the resolution of difficult lined or beaded objects.

"*Amphipleura pellucida* is easily resolved into delicate beads when the frustules are moderately coarse; and almost any that can be met with are resolvable into lines; and this, when these diatoms are mounted in balsam. The highest eye-pieces made may be used without any practical detriment to the image, although, of course, with a reduced sharpness of definition.

"On the whole, I think it in many cases the finest lens, of its power, that I have ever seen; and in every sense it is an admirable acquisition."

There is one drawback, viz., that the oil is a solvent of most varnishes and gums used in mounting and finishing slides (which may be remedied by coating the edge of the cover with shellac varnish); and also the necessity for the objects, such as frustules of diatoms, to be "burnt" on to the cover or mounted in balsam or other fluid with an equal refractive index.

MINERALOGY.

New Minerals from Laangban.—A number of new and interesting mineral species from this locality have been described by A. E. Nordenskjöld (*Jahrbuch für Mineralogie*, 1878, 206.) Atopite is the name given to a yellowish-brown or resin-brown, semi-translucent mineral which possesses the following composition:

Antimonic acid	72·61
Lime	17·85
Iron protoxide	2·79
Manganese protoxide	1·53
Potash	0·86
Soda	4·40
	100·04

The antimony appears to be present in the form of the higher oxide of that metal; and the analytical numbers correspond with the formula $2RO,Sb_2O_5$. Monimolite and romeite are the minerals most closely allied to the new species; it differs from the former in containing no lead oxide and a larger proportion of antimonic acid, and from the latter in the ratio of base to acid and in the fact that all the antimony is present in the form of antimonic

acid. Atopite is found at Laangban disseminated through hedyphane which occurs in fine veins and layers in rhodonite. It crystallizes in regular octahedra, and has a hardness of 5·5–6·0, and the specific gravity of 5·03.—Ekdemite is a coarsely crystalline mineral of a bright yellow colour with a faint greenish tinge; in thin plates it is translucent and is uniaxial and exhibits a distinct basal cleavage. The hardness is 2·5–3·0 and the specific gravity = 7·14. The solution of this mineral in acid reduces potassium permanganate. A pure specimen of ekdemite was found to possess the following composition:

Lead oxide	58·25
Lead	23·39
Chlorine	8·00
Arsenious acid	10·60
	100·24

These numbers indicate the formula $5PbO,As_2O_3 + 2PbCl_2$ as that of the new species. The mineral occurs, imperfectly foliated, in manganocalcite and as greenish yellow deposits incrusting it. The author also found at Laangban small lemon-yellow granules enclosed in the calcite which fills the cavities of a resin-brown coloured garnet; these granules contain arsenic, lead, and chlorine, but a satisfactory analysis of them could not be performed from want of material. He states that they crystallize in the rhombic system, and regarding them as ekdemite, he believes the new mineral to be dimorphous.—Hydrocerussite is a white mineral enclosing native lead; it appears to be a hydrated lead carbonate having the composition represented by the formula $2PbO,CO_2 + H_2O$. Hyalotekite is a white or pearl-grey, semi-translucent mineral, possessing a coarsely crystalline structure, and cleaving in two directions lying 90° or nearly 90° apart. It is easily fusible and is insoluble in hydrochloric acid and sulphuric acid. An imperfect analysis showed it to contain the following ingredients:

Silicic acid	39·62 per cent.
Lead oxide	25·30 „
Baryta	20·66 „
Lime	7·00 „
Loss on ignition	0·82 „
Alumina, potash, &c.	not determined.

In appearance it very closely resembles a greyish-white felspar, and it occurs in association with hedyphane and schefferite.—Ganomalite is the name which was given by the author in 1876 to a lead silicate occurring at Laangban. As it has recently transpired that certain specimens which were believed to be ganomalite contain no lead whatever, and are in reality tephroite (manganese silicate), Nordenskjöld has published fuller details and an analysis of the mineral. Ganomalite occurs massive, associated with tephroite, which it so closely resembles that any specimen under examination should be tested with the blowpipe. It is white or greyish-white, possesses marked doubly refractive power, has a hardness of 4·0, and the specific gravity of 4·98. When heated with soda in the reducing flame, the yellow crust on the charcoal and the reduced metal indicate the presence of lead. The composition of a specimen of this mineral was found to be:—

Silicic acid	34·55
Lead oxide	34·89
Manganese protoxide	20·01
Lime	4·89
Magnesia	3·68
Alkalies, &c.	1·86
	99·88

On one specimen, which appeared to be ganomalite in the crystallized condition, the angle between two planes of cleavage was found to be 104° 33′. The last of the minerals mentioned in this interesting paper has been termed jacobsite. It has strongly developed magnetic characters, and contains, in addition to some constituents which are apparently accidentally present, the following oxides:

Iron oxide	58·39 per cent.
Manganese oxide	6·96 „
Manganese protoxide	29·93 „
Magnesia	1·68 „

The formula of jacobsite appears therefore to be $MnO(Fe_2O_3, Mn_2O_3)$.

Adamite.—This mineral, a zinc arsenate, hitherto met with at Chanarcillo, Chili, and the Mine de la Garonne, has been found by Laspeyres in the calamine deposits of Laurium. It forms small crystals of a greenish hue, and occurs in the cavities of the rose-red or reddish massive ore of the Greek mines, associated with arseniosiderite, crystallized smithsonite, and sometimes with mimetesite. Its crystallographic characters were found not to differ materially from those of the specimens of adamite from Chanarcillo. Braun draws attention (*Jahrbuch für Mineralogie*, 1878, 188) to the interesting group of minerals, more than twenty in number, occurring in the Laurium beds, and points out the advantages which would accrue to science if they could be examined *in situ* by a good mineralogist. The ore which is now being raised is calcined before it is shipped and the minerals associated with the calamine are consequently destroyed.

The Separation of Minerals of Different Specific Gravities.—Church has recently called attention to the applicability of Sonstadt's solution to the separation of minerals of different specific gravity. The liquid consists of a solution of mercury iodide in potassium iodide, and is prepared by adding them alternately to the solution until no more of either is dissolved. A little free iodine occasionally colours the liquid, but this can be removed by the addition of some sodium hyposulphite. The light straw coloured liquid thus obtained may possess a specific gravity of 3·01 and can be employed for the separation of the mechanically loosened ingredients of any rock which it is desired to examine. Hardman describes the successful isolation by this means of a mineral occurring in a basalt of the North of Ireland. He used a solution having a specific gravity of 2·40, and was enabled to isolate two grammes of a mineral having a specific gravity of 1·70. This quantity the author questions his ability to have extracted after months of labour by any other method. He points out that it will now be possible to completely separate the three constituent minerals of granite—mica, felspar, and quartz, to weigh them and to determine almost absolutely their percentage, a problem which has as yet only been solved by mathematical calculation,

based on assumptions which can at best be merely approximately correct.—(*Chemical News*, 1878, xxvii. 108.)

In the current number of the *Mineralogical Magazine*, Prof. Heddle, of St. Andrews, draws attention to two properties of Sonstadt's "solution" which cannot too soon be impressed upon those who purpose to use it: it is a rapid and powerful vesicant, and it is exceedingly poisonous. It happened that some drops fell upon one of his hands, which was soon found to be in a state of violent inflammation. He does not regard the liquid as a "solution" in the strictest sense, but believes that a new salt is formed, which crystallizes in long needles apparently belonging to the oblique prismatic system; they have a highly dispersive power, a sulphur yellow colour, and are extremely deliquescent.—(*The Mineralogical Magazine*, 1878, ii. 63.)

Fluid Cavities in Blende.—A. Schortel has remarked the occurrence of a cavity, of nearly the size of a pea, and filled with liquid, in a specimen of Spanish blende. When the specimen was broken through the liquid was ejected. The walls of the cavity and the cleavage-faces were washed with distilled water and the transparent liquid qualitatively examined. The solution appears to have contained sodium chloride and zinc sulphate, the former predominating (*Berg. und Hüttenm.-Zeitung*, xxxvii. 49). Little cubes of salt have not unfrequently been observed in fluid cavities of other minerals.

A Société de Minéralogie has been formed in Paris. M. des Cloiseaux is president, and the meetings are held on the second Tuesday of each month in the Mineralogical Laboratory of the Sorbonne.

PHYSICS.

The Microphone.—It was announced early in May last that Professor Hughes, the inventor of the type-printing apparatus, had made the discovery that certain bodies placed in the circuit of a small battery were so affected by the sonorous vibrations of speech as to replace the transmitter of a Bell Telephone.

The instrument producing such remarkable results was described at the meeting of the Royal Society on the 9th of that month.

The telephone was employed as a phonoscope or receiving instrument of great delicacy, a small Daniel's battery being introduced into its circuit. It was first noticed that if a stretched wire were made part of this circuit, and the strain increased until it broke, a sound was heard at the moment of breaking, and repeated if the broken ends were pressed together.

"It was soon found that it was not at all necessary to join two wires endwise together to reproduce sound, but that any portion of an electric conductor would do so, even when fastened to a board or to a table; no matter how complicated the structure upon this board, or the materials used as a conductor, provided one or more portions of the electrical conductor were separated, and only brought into contact by a slight but constant pressure. Thus, if the ends of the wire terminating in two common nails laid side by side, and separated from each other by a small space, be electrically connected by laying a similar nail between them, sound could be repro-

duced. The effect was improved by building the nails log-hut fashion into a square figure, using ten or twenty nails. A piece of steel watch-chain acted well. Up to this point, the coarser vibrations alone were produced, the finer inflections were missing; in other words, the *timbre* of the voice was wanting, but in the following experiment it became more and more perfect, until it reached a perfection leaving nothing to be desired."

Mr. Hughes tried all forms of pressure and modes of contact. All metals could be made to produce identical results, provided the division was small enough, and that the material used did not oxidize by contact with the air filtering through the mass. A mass of shot is peculiarly sensitive to sound while clear, but as the shot soon becomes coated with oxide, this sensitiveness ceases. Carbon, from its surface being entirely free from oxidation, is excellent, but the best results at first attained were from mercury in a finely divided state. He used the willow-charcoal employed by artists for sketching, heating it gradually to a white heat, and then plunging it suddenly into mercury. The pores thus being filled with innumerable minute globules, held it in a state of fine division. Tin and zinc answered the same purpose. Pine charcoal thus treated with iron developed high conductive powers though of itself a non-conductor.

The substances were at first confined in a glass tube or box provided with wires for introducing them into the circuit.

The conductors were found to be affected by sounds absolutely inaudible except by this means, and hence the instrument was termed the Microphone.

In its later form it consists of a lozenge-shaped piece of gas-carbon 1 inch long, $\frac{1}{4}$ inch wide at the middle, and $\frac{1}{8}$ inch thick, the lower pointed end pivots upon a similar block; all these pieces of carbon being tempered in mercury.

The tube-transmitter exhibited to the Royal Society consisted of a glass tube 2 inches long and $\frac{1}{4}$ inch in diameter, containing four separate pieces of willow-charcoal pressed together till the resistance was about a third of that of the line in which it was to be employed.

Since then many and various forms have been adopted with success. A sphere of carbon, resting loosely between two hemispherical depressions in blocks of the same material answers well; and a pretty as well as a sensitive modification is made by Mr. Yeates of King Street, Covent Garden, consisting of a short cylinder of battery-carbon 1 inch long and $\frac{1}{4}$ inch diameter, pivoted loosely at its central point and resting by one end on a small carbon anvil, the pressure on which can be regulated, either by a superimposed weight or by a small spiral spring. All modifications are mounted on thin resonance-boards of pine, by which the aerial vibrations are caught and intensified as by the sound board of a violin.

By the kindness of Professor Hughes, the writer has been enabled to examine the last shape adopted. It consists of a small resonance box of light wood, like that of a tuning-fork, within which is a vibrating plate of iron about $1\frac{1}{4}$ inch long, and $\frac{1}{2}$ inch broad, pivoted, as in the previous case, about its centre, and held down on the contact-anvil by a light spring, capable of opposing a weight of from 5 to 20 grains. The contact pieces are of battery-carbon. If two of these microphones and two ordinary telephones be included in the same battery circuit, a curious fact comes to

light; for the two microphones being held to the lips of the communicating parties, and the telephones to their respective ears, each observer hears a note sung by the other, but not that which he is himself simultaneously emitting. The explanation of this undoubted phenomenon is somewhat obscure.

Dr. C. William Siemens, at a discussion before the Society of Telegraph Engineers, referred to the intimate connection between the microphone and its two elder sisters, the telephone and phonograph, in conjunction with which it formed a discovery which would probably be hereafter regarded as one of the greatest achievements in natural science of the present century.

He further compared the phonograph with the action of the brain in the exercise of memory, the external impressions being communicated to corpuscular bodies imbedded in grey substance, and connected with the nervous system of volition. Fresh impressions may be understood to give rise then and there to acts of will, but how stands the case with impressions communicated years before, and, as we term it, "Committed to Memory"? In order to this, the mind must have some power of reproducing a material record by which the impression has been rendered permanent. Such a hypothesis might also explain the confused images of dreaming, and the more vivid recollection of what occurred in early life, when the mechanical record may be supposed to have been distinct and indelible. It was also possible, by studying the action of selenium substituted for carbon in the microphone, that the impressions received by the retina would be equally susceptible of storage. The record itself might be supposed to be mechanical, more probably, molecular, at any rate, material.

Mr. Seabrooke points out that the microphone does not, strictly speaking, magnify the original sound, but substitutes for it a varying electric current of extreme rapidity. "In gently brushing the stand of the instrument, sound is heard in the telephone, but it does not at all follow that what we hear is a magnified reproduction of the brushing sound, for if the rapidity of the vibrations is insufficient to produce a sound, still they may move the carbon sufficiently to produce alternations of current, each of which may be able to set up vibrations of the telephone plate in its own period, or a modification of it."

Mr. F. J. M. Page introduces a Du Bois Reymond's induction coil by its primary into the microphonic circuit, the secondary coil being connected with a Lippmann's capillary electrometer, which immediately gives large and definite movements of its column.

Sir Henry Thompson demonstrated the surgical value of the microphone on June 4, in the Anatomical Theatre of University College. The sound used for detecting stone in the bladder was connected with the circuit, and whenever it struck the smallest fragment of calculus, the result was clear and unmistakable. Bullets and fragments of bone, he stated, might, by a similar process, be discovered.

On the day previous to Sir H. Thompson's demonstration, Mr. James Blyth brought before the Royal Society of Edinburgh a still simpler form of the microphone. He includes in the circuit "a small jelly can," half filled with cinders broken into coarse fragments, the connection being made by slipping down at opposite sides, between cinders and jar, two slips of tin

to which the wires are attached. Articulate sounds were thus heard very loud and distinct, though occasionally marred by the rattling of the cinders in the jar.

He then filled a shallow box of thin wood with cinders, nailing to its two ends tin connections. It proved to be a very sensitive transmitter, and if three such boxes were hung like pictures on the walls of a room almost any noise made in the room was revealed. A part song by two voices was distinctly transmitted. When water was poured into the original can, the Leclanché cell could be dispensed with.

It proved that the jar of cinders would act as a receiver as well as a transmitter. Two Grove's cells were included in circuit, and articulate sounds were clearly heard. The magnetic element of the telephone was here entirely superseded.

To demonstrate the actual motion and vibration of the cinders under the influence of sound, a strong battery and a glass jar were used. In a dark room flashes of electric light were observed between the fragments.

In a paper read before the Physical Society, Professor Hughes describes the "Physical Action of the Microphone" as the introduction into the circuit of an electrical resistance, which varies in exact accord with sonorous vibrations, so as to produce an undulatory current from a constant source. The undulations in length, height, and form, are an exact representation of the sonorous waves. It is essential that the instrument be so arranged as regards pressure between the touching surfaces as to be adapted to the particular vibrations employed. Thus a box suitable for a man's voice is not adapted to observe the tramp of a fly. When speaking, a galvanometer should be interposed in the circuit, and the pressure between the surfaces gradually increased from a minimum until the needle remains stationary, when a maximum loudness will be attained. In the experiments illustrating the paper, a small clock being placed on a drawing-board carrying a microphone, and made to interrupt the current passing through a telephone, the tick was audible throughout the room. The telephone being provided with a bell-mouthed tube, enabled the sounds to be heard at a distance. A second telephone was then introduced into the circuit, and laid on the board, a continuous sound being at once produced. Thus a relay for the human voice could be obtained; for it is only necessary to provide such an arrangement at each station for a speech to be received and transmitted to any number of succeeding stations. The system is perfectly duplex, for if two correspondents speak into microphones, and use telephones in receiving, each can hear the other, but his own speech is inaudible, and if each sing a different note, no chord is heard.

Messrs. Edwin Houston and Elihu Thomson, writing from the Central High School, Philadelphia, to "Nature" (June 28th), describe a "Telephone Relay" or "Repeater," which consists in applying the microphone directly to the diaphragm of the receiving telephone; the two fixed pieces of carbon being cemented to the diaphragm itself, and the third piece placed in cavities near their ends. The microphone forms part of the new circuit in which it is desired to repeat the message.

From some experiments on the *Influence of Temperature on the passage of air through capillary tubes*, Dr. Francis Guthrie concludes:—

(1.) That the time varies approximately as the square of the absolute temperature.

(2.) That the variation of the time deviates from the law of squares by a term approximately proportional to the temperature.

(3.) That the formula connecting the time and the temperature is nearly

$$t = d\mathrm{T}\ (\mathrm{T}+\beta)$$

where T is absolute temperature reckoned from — 273° C., and β seems to depend on $p_1 - p_2$ or the difference of pressures, the exact relation not being obvious.

Thermo-Electric Currents in Strained Wires have been investigated by Mr. G. W. von Tunzelmann. The object in view was to determine the conditions under which thermo-electric currents are produced in a circuit composed of a single metal, when one portion of the metallic conductor is subjected to a strain, and the strained and unstrained portions are maintained at different temperatures.

"Two tin cans were obtained, open at the top, and pierced at the bottom by necks, into which India-rubber corks were inserted; through slits in these the wires were passed. The wire was fastened by a clamp in the lower can, and grasped in the upper by a pair of wire-drawing dogs attached to the shorter arm of a lever, to the longer arm of which was attached another tin can, open at the top, and having at the bottom a neck fitted with an India-rubber tube, which could be closed by merely bending it up and hitching it in a hook attached to the can for that purpose. The strain on the wire was then produced as gradually as possible by pouring in measured quantities of shot. It could be removed as gradually by letting the shot run out by the India-rubber tube.

"The two cans through which the wire passed were kept, one at 100° C, the lower, at the surrounding temperature by a stream of water.

"The ends of the wire were connected with a Thomson's Galvanometer.

"As the strain was increased the current was increased, but only up to a certain limit. When it was carried beyond this point there was a gradual decrease in the current. If the strain was very carefully increased the direction of the current was reversed shortly before the breaking strain was reached."

Ten such experiments are recorded, from one of which a diagrammatic curve is obtained.

On the pitch of a tuning-fork in an incompressible fluid, by Felix Auerbach. When a tuning-fork is plunged in water, a note, not agreeing with that in air, is given out, the dissipation of kinetic energy taking place in incompressible fluid differently from that occurring in air. The tone becomes deeper in the ratio $\sqrt{1{\cdot}4} : 1$, *i.e.*, as 1·18 : 1, or about as 7 : 6, more, therefore, than a tone, and less than a minor third.

The fall in pitch was determined by means of beats between a fork immersed in water and another, of a tone lower, in air. It appears that the resistance of liquids depending on density and viscosity does not come sensibly into consideration as regards wave lengths, though it does as regards

amplitudes. This is true of mercury and glycerine, though the former is of high density, and the latter of great viscosity.

Experiments with Floating Magnets, made by Alfred M. Mayer, show the motions and arrangements of freely moving bodies acted on by forces of attraction and repulsion.

A dozen sewing needles, magnetized with their points of similar (N) polarity, are run into small corks so as to float upright in water, with the eyes above the surface.

The north pole of a large cylindrical magnet being brought down over them, they arrange themselves as an equilateral triangle. Four needles form a square, or an equilateral triangle with a central needle. Five form a square with centre, or a pentagon; six a pentagon with centre, or an equilateral triangle of six points arranged quincuncially; seven form a regular hexagon with central needle, and so on.

Floating needles may be used generally as delicate indicators of magnetic actions, such as the position of poles, and the displacement of lines of magnetic force during inductive action, or, for instance, in the telephone.

ZOOLOGY.

Aerial Respiration in Fishes.—Professor Jobert, of Dijon, who is now engaged in making some zoological investigations in Brazil, at the instance of the Emperor Don Pedro, has ascertained some exceedingly remarkable facts in connection with the respiration of certain fishes. A Siluroid fish, which inhabits the neighbourhood of Rio de Janeiro (*Callichthys asper*), and is noted for its power of living a long time out of the water, was found by him to swallow small portions of air, from which it partly absorbs the oxygen by the agency of the walls of the intestinal canal, the carbonic acid formed, and the unabsorbed nitrogen passing away by the anal aperture. On examining the structure of the intestine, Professor Jobert found its inner surface bearing a multitude of filiform appendages arranged in tufts, and composed essentially of blood-vessels.

A somewhat analogous case was observed in several other fishes inhabiting the valley of the Amazon. They live in stagnant water, the temperature of which often exceeds 104° F.; but this does not appear to be sufficient to support their respiration, and they are obliged to come frequently to the surface for a supply of air. Sometimes, also, the water in which they have been living is dried up, when they are seen making considerable journeys by land in search of more favourable localities, crawling on the ground by means of their pectoral fins. Some of these are species of *Callichthys*, and, like the *C. asper* of Rio de Janeiro, they possess a double respiration,—respiring the air contained in the water surrounding them by means of their gills, and also the atmospheric air which they swallow, and which passes through their intestine. The escape of the exhausted air from the anal aperture in these fishes is said to produce a constant bubbling in the water they inhabit, and M. Jobert's investigations, though imperfect, sufficed to convince him that the air evacuated contained much carbonic acid, and less oxygen than atmospheric air. The vascular tufts clothing the wall of the intestine originate from

adjacent veins, in the same way as the afferent vessels of a lung. Species of *Doras* and *Hypostomus*, inhabiting the Upper Amazon, respire air in the same way as the *Callichthyes*, but in the *Hypostomi* the used air is returned towards the mouth, and escapes by that orifice or by the branchial apertures.

In *Sudis gigas* and some species of *Erythrinus* aerial respiration takes place by the agency of the swimming-bladder, which, in the latter, has long been known to communicate with the outer world through the œsophagus, and to be furnished internally with numerous cells formed by membranous folds. Professor Jobert finds that the walls of the swimming-bladder, including all these folds, are richly provided with blood-vessels, mostly originating from the venous system, and that it is thus converted into a true lung, by the possession of which the fishes are enabled to live for a long time out of the water. Of the reality of this respiration, Professor Jobert convinced himself experimentally by obstructing the air-duct leading to the bladder; the fish soon died by suffocation. These observations are particularly interesting as establishing further bonds between the true fishes the Lepidosirens, and the perennibranchiate Batrachians, which possess at the same time branchiæ and true lungs.—(*Comptes rendus*, April 15, 1878.)

A New Tubicolar Annelid.—The late Professor Claparède described certain small Annelids as stages in the development of the well-known *Terebella conchilega*. M. Adolphe Wartel, a pupil of Professor Giard's, detected what appear to be the same Annelids attached to the branches of the common Campanularian polype, *Laomedea gelatinosa*, where they inhabit small projecting transparent tubes, so exactly resembling the gonothecæ of the polype, as easily to escape detection. The presence of generative products in many of them proved them to be adult forms, and not young stages of any *Terebellæ*; and Professor Giard establishes a new genus and species for this interesting little Annelid, which he names *Wartelia gonotheca*, the generic name commemorating its discoverer, the specific alluding to the curious mimicry of the gonothecæ of the Hydroid which its tubes present. It is remarkable for the presence of large otocysts like those of mollusca and for certain peculiarities of the ventral cirri, which lead to its removal far from the *Terebellæ*.—(*Comptes rendus*, May 6, 1878.)

Circumspection of Ants.—Professor Leidy gives somes curious details concerning the little red ant which seem to show that this insect is capable of becoming circumspect by experience. When he purchased his present residence, and while it was undergoing some repairs, he noticed a piece of bread, left by a workman in one of the rooms, swarming with ants. Fearing that the house was seriously infested by these insects, which, when numerous, are great pests, to ascertain whether it was so he placed a piece of sweet cake in every room from cellar to attic, and at noon every piece of cake was covered with ants, and each piece was carefully picked up, and the ants tapped into a cup of oil of turpentine. The cake was replaced, and in the evening was found again covered with ants. The same process was repeated morning, noon, and night for the next two days; on the third day the number of ants was greatly diminished, and on the fourth there were none. Professor Leidy at first supposed that all the ants had been destroyed, but in the attic he observed a few feasting on some dead house flies, which led him to the conclusion that the remaining ants had become suspi-

cious of the sweet cake. He accordingly distributed through the house pieces of bacon, which were soon swarming with ants. This was repeated with the same result for several days, when the ants finally ceased to visit the bacon. Pieces of cheese were next tried with the same results, but with an undoubted thinning in the multitude of ants. When the cheese no longer proved attractive, recollecting the feast on dead flies in the attic, dead grasshoppers were supplied from the garden. These again proved too much for the ants; and, after a few days' trial, neither grasshopper nor anything else attracted them: they appear to have been thoroughly exterminated, nor has the house since been infested by them.—(*Proc. Acad. Nat. Sci. Philad.*, 1877.)

A Pigeon Living without a Brain.—Dr. McQuillen described before the American Philosophical Society (Feb. 1, 1878), a case of the extirpation of nearly the whole of the cerebrum of a pigeon, operated upon by himself. He desired to place on record the fact that the animal not only survived the operation twenty-four days, but that it gradually regained its usual powers and habits of flight and its ability to feed itself and drink. Only one other such case is on record.

The Turbellaria of the Deep Waters of the Lake of Geneva.—M. Duplessis finds that, with two exceptions, all the species of Turbellarian worms from the bottom of the Lake of Geneva are also found in the stagnant waters of the shores, or in the marshes and small lakes of other parts of the canton, but that most of the species have undergone some modifications. Thus specimens of the Planarians *Dendrocœlum lacteum* and *fuscum*, from the deep waters, are generally smaller and lighter in colour than those found in shallow water, and have the digestive tube of a rosy tint. The visual organs tend to become atrophied; but one variety of *Dendrocœlum lacteum* has each eye-spot divided into two smaller points, and has been described as a distinct species under the name of *Planaria quadrioculata*. *Microstomum lineare*, a Rhabdocœlan form, becomes larger, and has the intestine of a pale rose colour when living at the bottom of the lake.

The two exceptions above mentioned are very remarkable ones. While the rest of the deep-water Turbellarians all belong to the neighbourhood, these two species, which have been named *Vortex lemani* and *Mesostomum morgiense*, but which probably represent new genera, resemble Mediterranean types, and the second belongs to a group of Turbellaria hitherto regarded as exclusively marine. These two forms, which have been found in one or two other European lakes, are among those which descend to the greatest depth.

As analogous phenomena M. Duplessis refers to the occurrence among the Crustaceans of the deep fauna of a form related to the marine genus *Cythere*; and states that he has himself obtained two species of Arachnida (mites), closely related to marine forms. One of these is a *Campoynatha* (*C. Forelli*) which so nearly resembles a small species found on the shores of the Mediterranean, that the two might easily be confounded; the other belongs to a genus hitherto known only from the Mediterranean. These facts are exceedingly interesting and important, in connection with other observations which seem to prove the survival in the deepest waters of many lakes of a residue of the old marine fauna which once occupied them. —(*Bibl. Univ., Arch. des Sci.*, Oct. 15, 1877).

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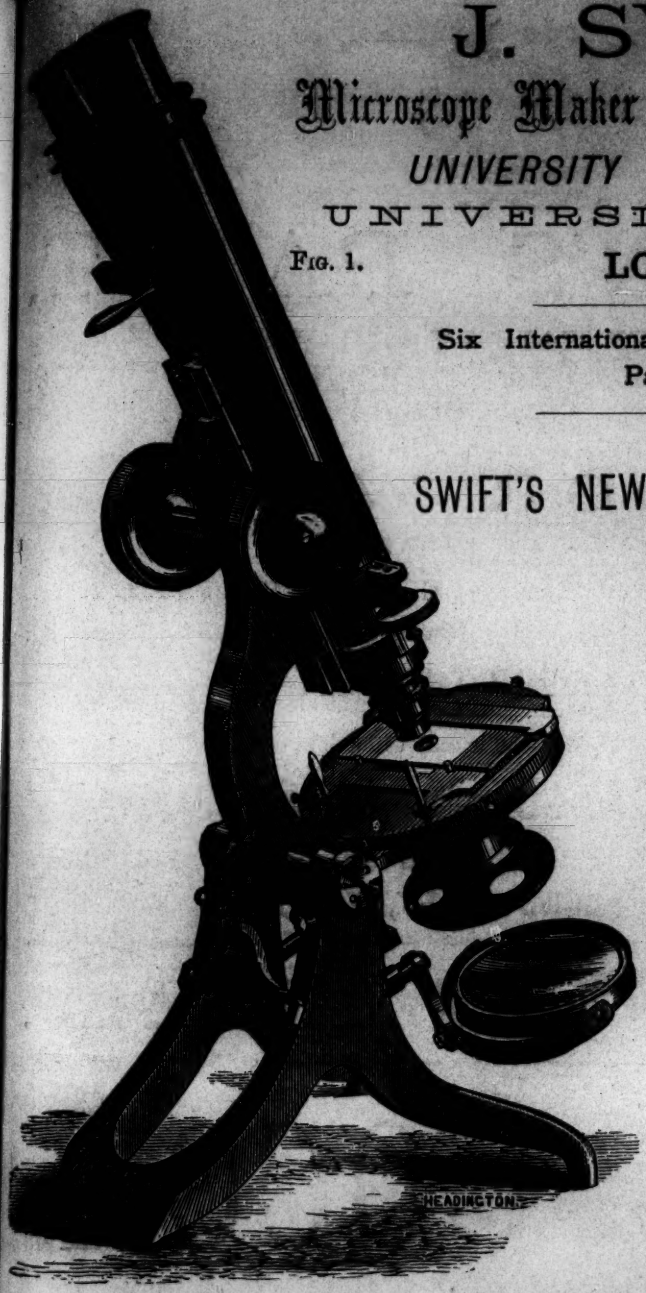

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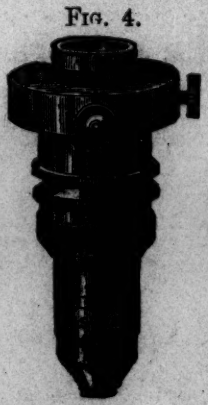

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